CANADIAN JOURNAL OF PSYCHOLOGY

VOLUME 15, No. 2 JUNE, 1961

PUBLISHED FOR THE
CANADIAN PSYCHOLOGICAL ASSOCIATION BY THE
UNIVERSITY OF TORONTO PRESS

AUTHORIZED AS SECOND-CLASS MAIL, POST OFFICE DEPARTMENT, OTTAWA

CANADIAN JOURNAL OF PSYCHOLOGY

Editor: JULIAN BLACKBURN
Assistant Editors: P. H. R. JAMES, P. C. DODWELL

THE CANADIAN JOURNAL OF PSYCHOLOGY is published quarterly in March, June, September, and December. Annual subscription, $6.00, single number, $1.75.

Subscriptions. Orders and correspondence regarding subscriptions, change of address, and purchase of back numbers should be sent to:

THE SECRETARY-TREASURER, CANADIAN PSYCHOLOGICAL ASSOCIATION

Box 31, Postal Station D., Ottawa, Ontario

Contributions. Original manuscripts and correspondence on editorial matters should be sent to:

THE EDITOR, CANADIAN JOURNAL OF PSYCHOLOGY

Queen's University, Kingston, Ontario

Information for contributors. The *Journal* publishes experimental and theoretical articles in all recognized fields of psychology. Contributors need not be members of the CPA or residents of Canada. Manuscripts, either in English or in French, should be submitted in duplicate, they should be double-spaced throughout, and they should follow standard practice as regards tables, figures, etc. Authors are asked to adopt the current practice in American and British Journals of citing references by date, for example, Smith (1956), instead of by number. "Immediate publication" (i.e. in the next issue to go to press) can be arranged for authors willing to pay the extra costs involved.

CANADIAN PSYCHOLOGICAL ASSOCIATION, 1960–61

President: R. B. BROMILEY, Toronto; *Past President*: G. H. TURNER, London; *President Elect*: R. B. MALMO, Montreal; *Secretary-Treasurer*: F. R. WAKE, Ottawa; *Employment Bureau*: MISS MARJORIE FAHRIG, 68 Tower Road, Ottawa 5.

The Canadian Psychological Association also publishes *The Canadian Psychologist*, the business and professional quarterly of the Association. Editor: W. R. N. BLAIR, 462 Melbourne Ave., Ottawa 3, Ont. Annual subscription, $3.50; single number, $1.00. Correspondence regarding subscriptions should be sent to the Secretary-Treasurer of the Association. Contributions should be addressed to the Editor.

INFLUENCE D'UNE STIMULATION ÉLECTRIQUE SUR LE NIVEAU D'ACTIVATION ET LA PERFORMANCE[1]

RAYMOND DUCHARME ET DAVID BÉLANGER
Institut de Psychologie, Université de Montréal

UN DES POSTULATS FONDAMENTAUX de la théorie de Hull (1943) veut que l'intensité du comportement dépende conjointement des facteurs d'habitude (H) et de tendance générale (D). Les modifications de certaines conditions antécédentes, telles la prolongation d'une privation d'eau ou de nourriture et l'accroissement d'une stimulation, agiraient directement sur le niveau de tendance dont l'élévation se manifesterait par l'intensification de l'activité spécifique à la situation dans laquelle se trouve placé l'organisme en cause. Pourtant, les résultats de plusieurs expériences ne confirment pas cette prédiction. On a souvent démontré, par exemple, qu'une privation prolongée peut provoquer une diminution du nombre de réponses instrumentales (Finan, 1940; Brush, 1957; Stennett, 1957; Malmo, 1959; Bélanger, 1959). Il convient donc de se demander si toute privation ou stimulation, pour peu qu'elle dépasse une certaine intensité, ne conduit pas à un état psychophysiologique qui entraîne toujours un fléchissement de l'activité instrumentale. Le but de ce travail est de déterminer l'effet d'une stimulation électrique d'intensité croissante sur la fréquence cardiaque et sur le taux de réponses instrumentales des rats placés dans une situation de type Skinner.

Les hypothèses sur lesquelles s'appuie cette recherche découlent de plusieurs études récentes sur la stimulation électrique et la privation d'eau. Siegel et Siegel (1949) ont étudié l'effet de chocs électriques sur l'absorption d'eau. Des rats légèrement assoiffés recevaient un choc électrique avant d'être replacés dans leur cage, où on leur présentait de l'eau. La quantité d'eau absorbée par ces animaux durant une période subséquente de deux heures était significativement plus considérable que chez un groupe témoin qui n'avait pas été exposé au stimulus nocif. Amsel et Maltzman (1950) ont obtenu des résultats à peu près identiques, tandis que Siegel et Brantley (1951), en utilisant la nourriture au lieu de l'eau dans une expérience analogue, ont retrouvé la même relation. Récemment, l'un de nous (Bélanger, 1959) a constaté que des privations d'eau de 60 et 72 heures suscitaient une diminution des réponses instrumentales, alors que des privations allant de 12 à 24, 36 et 48 heures avaient plutôt pour effet de faire augmenter le nombre de pressions sur

[1]Cette recherche a été rendue possible grâce à un octroi (APBT-1) du Conseil National de Recherches du Canada.

le levier d'une boîte de Skinner. Par ailleurs, le rythme cardiaque de ces animaux s'accélérait graduellement avec la durée de la privation pour atteindre un maximum à 72 heures, de sorte qu'il est impossible d'invoquer l'hypothèse de l'inanition pour expliquer la chute de l'activité après 60 et 72 heures de privation.

Ces données laissent supposer qu'une stimulation électrique peut avoir les mêmes effets qu'une privation d'eau. A l'intérieur de certaines limites, toute augmentation d'intensité du stimulus provoquera une activation physiologique qui se manifestera par l'accélération du rythme cardiaque et l'accroissement du taux de réponses instrumentales. Quand l'intensité du choc dépassera le point optimal cependant, l'activité physiologique et la fréquence cardiaque iront toujours en s'accroissant, mais le nombre de réactions adaptées à la situation diminuera graduellement. En d'autres termes, la fréquence cardiaque augmentera de façon continue et proportionnelle avec l'intensité du choc électrique, alors que le rapport du nombre de pressions sur le levier à l'intensité de la stimulation s'exprimera par une courbe non-linéaire en forme de U inversé.

Méthode

L'expérimentation a porté sur 31 rats mâles albinos, de même âge et de même stock (Sprague-Dawley). Ces animaux ont d'abord appris à se procurer de l'eau en appuyant sur un levier dans une boîte de Skinner. L'appareil était un modèle récent de type commercial (Davis, DB-103), constitué essentiellement d'une boîte de forme rectangulaire (9" × 12" × 13") avec des murs de matière plastique transparente. Un levier disposé au centre d'un de ces murs, à 3½ pouces du plancher, déclenchait un mécanisme qui servait à mettre une goutte d'eau à portée de l'animal. A chaque jour, privés d'eau depuis environ 23 heures, les rats étaient déposés dans la boîte pendant 6 minutes. Après un mois d'entraînement de ce genre, ils avaient appris à presser sur le levier à un rythme stable et régulier.

Vinrent ensuite l'implantation des électrodes et une série de séances destinées à familiariser l'animal avec le montage technique nécessaire à l'enregistrement de la fréquence cardiaque. Après avoir soumis l'animal à une légère anesthésie à l'éther, on lui a implanté deux bouts de fil chirurgical métallique inoxydable dans la peau, au niveau de chaque épaule. Les deux extrémités de ces électrodes restent cachées dans le poil de l'animal et, au moment de l'enregistrement, sont reliées aux fils de l'électrocardiographe (Sanborn, modèle 51) au moyen de deux petites pinces. Lorsque le rat est placé dans la boîte, les fils de l'électrocardiographe sont suspendus au-dessus de lui de façon à ne pas gêner ses mouvements. Les animaux se sont rapidement adaptés à la situation et, une fois le rythme cardiaque complètement stabilisé, l'expérience définitive a débuté.

Pour cette série d'épreuves finales, toujours faites après une privation d'eau de 23 heures, les rats étaient d'abord déposés dans une cage spéciale où ils devaient recevoir la stimulation électrique. L'animal demeurait dans cette cage 15 minutes, après quoi il recevait un choc électrique d'une durée de 5 secondes. Il était ensuite transporté le plus rapidement possible dans la boîte de Skinner. L'intervalle requis pour ce changement de cage et pour la mise en place des fils enregistreurs a varié entre 25 et 40 secondes. Tous les rats furent soumis à une épreuve de six minutes

pendant sept jours consécutifs, l'intensité de la stimulation passant d'un jour à l'autre de .00 à .08, .16, .24, .32, .40 et .60 milliampères. A chaque séance, on prenait cinq échantillons de la fréquence cardiaque, soit au début de chacune des cinq dernières minutes de l'épreuve. On a enregistré également le nombre de pressions exercées sur le levier durant chacune des six minutes d'activité. Au début de l'épreuve finale, le poids moyen des rats était de 380 grammes, avec une dispersion de 305 à 455 grammes.

Résultats

La fréquence cardiaque augmente, tel que prévu, avec l'intensité de la stimulation électrique. Le nombre moyen de pulsations à la minute passe en effet graduellement de 403 à la première séance (aucune stimulation électrique) à 461 à la dernière journée (choc d'une intensité de 0.60 m.amp.). La figure 1 résume graphiquement ces résultats. L'ensemble

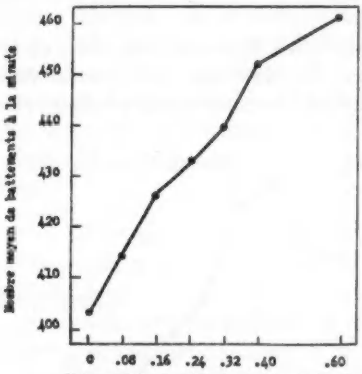

Figure 1. Variation de la fréquence cardiaque par rapport à l'intensité du choc électrique.

des données sur la fréquence cardiaque ont été soumises à une analyse de variance suivant un devis de deux facteurs avec mesures répétées. Les résultats de cette analyse (Tableau I) indiquent que la fréquence cardiaque varie de façon significative (niveau 1 pour cent) avec l'intensité de la stimulation et avec le moment de l'enregistrement. Toutefois, l'absence d'interaction entre ces deux facteurs permet d'étudier indépendamment les données relatives au rapport entre la fréquence cardiaque et l'intensité du choc électrique. Pour vérifier directement l'hypothèse portant sur la linéarité de ce rapport, on a procédé à l'analyse de la conformation de la courbe (Grant, 1956).

TABLEAU I

ANALYSE DES DONNÉES SUR LA FRÉQUENCE CARDIAQUE

Source de variation	Somme des carrés	Degré de liberté	Carré moyen	F
Entre les sujets	1298692.4	30		
Stimulation	387190.1	6	64531.6	108.6*
Moment de l'enregistrement	26886.4	4	6721.6	11.3*
S X M	18977.1	24	790.7	1.3
Résiduel	606245.6	1020	596.3	
Total	2337991.6	1084		

*Niveau de signification 1 pour cent.

On trouve une composante linéaire significative au niveau 1 pour cent ($F = 99.49$). Par ailleurs, cette fonction ne comporte aucune composante quadratique significative.

La relation qui existe entre l'intensité du choc et la fréquence des réponses instrumentales s'exprime par une courbe représentée par la figure 2. Les deux premières intensités de stimulation (.08 et .16 m. amp.)

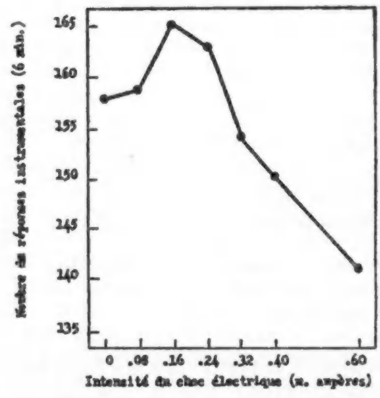

FIGURE 2. Rapport entre le nombre moyen de pressions sur le levier et l'intensité du choc électrique.

provoquent d'abord un accroissement sensible de l'activité instrumentale, mais ensuite, plus le choc est intense, plus le nombre de pressions sur le levier diminue. Le même schème d'analyse de variance à deux facteurs a été appliqué aux données sur les réponses instrumentales (Tableau II).

TABLEAU II

ANALYSE DES DONNÉES SUR LE NOMBRE DE RÉPONSES INSTRUMENTALES

Source de variation	Somme des carrés	Degré de liberté	Carré moyen	*F*
Entre les sujets	27512.15	30		
Stimulation	2163.29	6	360.54	6.41*
Moment de l'enregistrement	16735.33	5	3347.06	59.53*
S X M	1228.79	30	40.95	
Résiduel	69151.77	1230	56.22	
Total	116791.33	1301		

*Niveau de signification 1 pour cent.

Le nombre de pressions exercées sur le levier pendant la période de six minutes varie d'une façon significative (niveau 1 pour cent) avec l'intensité de la stimulation et avec le moment de l'enregistrement. Encore ici, ces deux facteurs n'entrent pas en interaction. L'analyse de la forme de la courbe représentant la relation entre l'intensité de la stimulation et l'activité instrumentale, vérifie l'hypothèse de la curvilinéarité: la composante quadratique de cette courbe est significative au niveau 1 pour cent ($F = 9.16$), alors que sa composante linéaire ne l'est pas ($F = 3.61$). Les deux hypothèses posées au début de cette expérience se trouvent donc vérifiées.

DISCUSSION ET INTERPRÉTATION

Pour expliquer l'augmentation de l'absorption d'eau à la suite de chocs électriques, Siegel et Siegel (1949) invoquent un phénomène d'hémoconcentration, qui entraînerait une déshydratation des tissus. Cette déshydratation susciterait chez l'animal une prise de conscience de la dessiccation buccale et, partant, de la soif. Cette interprétation se conforme aux théories classiques suivant lesquelles la fréquence d'une activité dépend de l'intensité du besoin correspondant.

Ce type d'explication ne semble pourtant pas s'appliquer aux résultats rapportés ici. Même si ce processus de déshydratation avait réellement joué dans le cas des animaux de Siegel et Siegel, les faits observés dans la présente expérience ne permettent pas de faire appel à une telle hypothèse. La stimulation utilisée par Siegel et Siegel durait 45 secondes et était assez intense pour provoquer la défécation du rat. Dans le cas présent, le choc électrique ne dure que 5 secondes et l'intensité suffisante à entraîner la réaction instrumentale maximum n'était que de 0.16 milliampères. D'ailleurs, même s'ils avaient lieu, ces changements osmotiques ne sauraient être assez rapides pour déterminer une augmentation de l'activité quelques minutes seulement après la stimulation. Enfin –et c'est là où l'explication de Siegel et Siegel vient en contradiction nette

avec les résultats rapportés plus haut—plus l'intensité du choc augmente à partir de 0.16 milliampères, plus l'activité instrumentale diminue.

De toutes façons, que le choc électrique suscite ou non un besoin organique, il faut trouver le mécanisme par lequel l'animal réagit à cette stimulation. On a pu le constater, la variation de l'intensité du choc électrique exerce sur la fréquence cardiaque et sur l'activité instrumentale le même effet que l'augmentation graduelle d'une privation d'eau (Bélanger, 1959). En posant cette hypothèse avant d'entreprendre l'expérience, on s'appuyait sur le postulat que le niveau d'activation peut être modifié de plusieurs façons: soit, par exemple, directement par la variation d'une privation déclenchant une tendance appropriée à la situation expérimentale, soit indirectement en provoquant par une stimulation extérieure une mobilisation d'énergie capable de s'ajouter au faible potentiel déjà évoqué par une légère privation d'eau. Le concept de mobilisation d'énergie invoqué ici correspond, selon Duffy (1957), à celui d'activation. Il s'agit d'un mécanisme très simple: le choc électrique constitue une stimulation sensorielle soudaine et très caractéristique qui suscite dans le système réticulaire un état d'activation plus ou moins intense. L'excitation neurale qui en découle emprunte les voies efférentes disponibles et produit, par le fait même, des tensions musculaires dans tout l'organisme. A cause du phénomène de rétroaction (feedback), cette excitation persiste pendant un certain temps, de sorte qu'il est possible de parler d'une accumulation d'énergie.

Dans l'expérience rapportée ici, l'animal reçoit d'abord un choc électrique, puis on le dépose dans la boîte de Skinner. L'expérience antérieure a créé chez lui une habitude ou, si l'on préfère, lui a fourni des "connaissances," qui donnent à cette situation une signification capable de déterminer son comportement. Privé d'eau depuis 23 heures, le rat se trouve donc en présence d'un stimulus approprié (le levier) vis-à-vis duquel il est "sensibilisé" (Malmo, 1959) et dont la perception détermine l'actualisation du potentiel énergétique accumulé grâce à la stimulation électrique précédente. Les variations de la fréquence cardiaque accompagnant cette décharge énergétique sont considérées comme un indice physiologique du niveau d'activation qui croît directement avec l'intensité du choc électrique. Toujours selon la théorie de l'activation, la perception du stimulus approprié provoquerait en même temps une intensification du bombardement du cortex cérébral par les influx provenant du système réticulaire ascendant. La régulation du comportement dépendrait de cette activité corticale: les bombardements corticaux favoriseraient l'activité instrumentale jusqu'au point où, devenus trop intenses, ils aboutiraient à une désynchronisation qui se manifesterait par une diminution des réponses instrumentales (Hebb, 1955), d'où la courbe en

U inversé caractéristique des situations où le niveau d'activation s'accroît démesurément.

Pour expliquer l'augmentation de l'activité des rats avec l'intensification du choc électrique, la théorie de l'activation n'est donc pas forcée d'avoir recours à l'influence d'un besoin spécifique, comme celui de la déshydratation des tissus. Qu'il s'agisse de stimuli rattachés spécifiquement à une tendance appropriée (Bélanger, 1959) ou de stimuli purement externes n'entretenant aucun lien avec la tendance appropriée comme dans le cas présent, le mécanisme d'activation est le même et le résultat final, du moins en ce qui concerne les variations du rythme cardiaque et de l'activité instrumentale, est équivalent. Bien plus, même dans le cas d'une motivation inappropriée, on a obtenu des résultats qui font conclure à l'existence d'un effet positif de généralisation d'une tendance inappropriée (Bélanger & Tétreau, 1961). Cet effet se traduirait par une hausse du niveau d'activation dont le mécanisme ne différerait de celui décrit ici que par la nature des stimuli qui le mettent en branle.

Il est donc permis de conclure que, contrairement à la notion de besoin, l'activation n'est pas spécifique à chaque activité. Elle englobe d'ailleurs cette notion de besoin, puisque tout besoin organique peut agir sur le niveau d'activation (Morgan, 1959). Il s'agit d'un phénomène général que l'on retrouve dans une variété de situations.

Summary

Thirty-one rats were trained to press on a lever for water reinforcement. After this training, they were given electric shocks of increasing intensity on each of seven consecutive days immediately before being placed in the Skinner box for a six-minute period during which heart rate and number of instrumental responses were recorded. At the moment of testing, on each day, the animals were under a 24-hour water deprivation. The HR increased continuously with the intensity of stimulation. The number of presses also increased with the intensity of shock but decreased continuously with intensities above 0.16 m.amp. These results are compared with previous experiments and interpreted in the context of an activation theory of motivation.

RÉFÉRENCES

Amsel, A., & Maltzman, I. The effect upon generalized drive strength of emotionality as inferred from the level of consummatory response. *J. exp. Psychol.*, 1950, **40**, 563–9.

Bélanger, D. Les relations entre la fréquence cardiaque du rat et les niveaux de tendance et d'activation. Thèse de doctorat, Université de Montréal, 1959.

Bélanger, D. & Tétreau, B. L'influence d'une motivation inappropriée sur le comportement du rat et sa fréquence cardiaque. *Canad. J. Psychol.*, 1961, **15**, 6–14.

Brush, F. R. The effects of shock intensity on the acquisition and extinction of an avoidance response in dogs. *J. comp. physiol. Psychol.*, 1957, **50**, 547–52.

DUFFY, ELIZABETH. The psychological significance of "arousal" or "activation." *Psychol. Rev.*, 1957, **64**, 265–75.

FINAN, J. L. Quantitative studies in motivation. I. Strength of conditioning in rats under various degrees of hunger. *J. comp. Psychol.*, 1940, **29**, 119–34.

GRANT, D. A. Analysis-of-variance tests in the analysis and comparison of curves. *Psychol. Bull.*, 1956, **53**, 141–54.

HEBB, D. O. Drives and the C.N.S. (conceptual nervous system). *Psychol. Rev.*, 1955, **62**, 243–54.

HULL, C. L. *Principles of behavior.* N.Y.: Appleton-Century, 1943.

MALMO, R. B. Activation: a neuropsychological dimension. *Psychol. Rev.*, 1959, **66**, 367–87.

MORGAN, C. T. Physiological theory of drive. In S. Koch, ed., *Psychology: a study of a science.* New York: McGraw-Hill, 1959, I, 644–71.

SIEGEL, P. S., & BRANTLEY, J. J. The relationship of emotionality to the consummatory response of eating. *J. exp. psychol.*, 1951, **42**, 304–6.

SIEGEL, P. S., & SIEGEL, H. S. The effect of emotionality on the water intake of the rat. *J. comp. Physiol. Psychol.*, 1949, **42**, 12–16.

STENNETT, R. G. The relationship of performance level to level of arousal. *J. exp. Psychol.*, 1957, **54**, 54–61.

(Reçu le 12 août, 1960)

STRAIN DIFFERENCES IN THE RETROACTIVE EFFECTS OF ELECTROCONVULSIVE SHOCK ON MAZE LEARNING[1]

CALVIN W. THOMSON, JAMES L. McGAUGH, CHARLES E. SMITH, WILLIAM J. HUDSPETH, AND WILLIAM H. WESTBROOK
San Jose State College

Rosenzweig, Krech, and Bennett (1960) have reported that the descendants of the Tryon maze-bright strain (S_1) of rats are superior to descendants of the Tryon maze-dull strain (S_3) in several learning tasks. Recent studies in our laboratory (McGaugh, Westbrook, & Burt, in press; Jennings, 1960) have replicated their finding that the S_1 rats are consistently superior to the S_3 rats in performance on the Lashley III maze. In all of these studies the training conditions have consisted of two or more (typically 5) trials a day with an intertrial interval of 30 seconds. Recent findings have indicated, however, that the strain differences in learning typically found with these strains are not obtained when the trials are distributed (Breen & McGaugh, in press; Jennings, 1960; McGaugh, Westbrook, & Ross, 1960) or when subjects of the two strains are injected with the neural stimulant 5-7-diphenyl-1-3 diazadamantan-6-01 (1757 I.S.) (McGaugh, Westbrook, & Burt, 1960). Under both of these conditions the S_3 rats perform as well as the S_1 ones. Rowland and Woods (1961) have even found evidence of S_3 superiority when only one trial a day is given in a 17-unit maze.

These findings have led us to suggest (McGaugh, 1960) that instead of saying that these two strains differ in a general adaptive or learning capacity, we can make the more specific statement that they differ in the rate of post-trial consolidation of neural processes mediating maze behaviour. The maze-brights are superior to the maze-dulls when trials are massed, according to this hypothesis, because of a faster rate of neural consolidation between each learning trial. Distribution of practice, according to this hypothesis, eliminates strain differences in learning by allowing maximum consolidation in rats of *both* strains. And, finally, neural stimulants equalize learning when trials are massed by facilitating neural activity in S_3 rats, thus diminishing or eliminating the strain difference in consolidation rate. This last interpretation makes the further assumption, of course, that neural stimulants would have a greater effect on the S_3 animals than on the S_1 animals.

[1]This research was supported by Research Grant MY-3541, from the National Institute of Mental Health, Public Health Service. We would like to express our appreciation to Professor David Krech for his assistance and advice, and to the Berkeley Laboratory for their co-operation in supplying us with S_1 and S_3 breeders.

Canad. J. Psychol., 1961, 15(2)

The effect of electroconvulsive shock (ECS) on learning provides still another avenue to test our general hypothesis. A number of studies (for example, Duncan, 1949; Leukel, 1957; Thompson & Dean, 1955) have shown that, in rats, retention is impaired by post-trial administration of electroconvulsive shock and that the degree of impairment is less with longer delays between learning and the ECS treatment. In the present study, rats of the S_1 and S_3 strains were given ECS treatments after each maze-learning trial. It was hypothesized that if the S_1 and S_3 strains differ in rate of consolidation, then the post-trial ECS treatments should have differentially disruptive effects on the learning of rats from the two strains, with the S_3 animals being more severely affected.

Method

Subjects

Sixty-five naïve S_1 (28 males, 37 females) and 75 naïve S_3 (35 males, 40 females) animals of 80 to 90 days of age were used in the study.

Apparatus

A 39-in. pre-training straight runway and a 4-unit, 8-cul Lashley III alley-maze were used. Each maze alley was 4 in. wide, 5 in. high, and 47 in. long. The runway and the maze were painted medium gray and were covered with ½ in. hardware cloth. The floor of the maze was marked with white lines 2 in. from both sides of each choice point.

Procedure

All Ss were first handled, pre-trained to run to a wet mash reward in the straight alley, and were brought to 85–90 per cent of normal body weight. A total of 18 runway trials was given each S over an 8-day pre-training period. Then Ss from each strain were assigned at random to one of 7 groups—6 experimental groups and 1 control group—and were given 1 trial a day for 8 days in the Lashley III maze. An error was recorded each time an S extended his head beyond one of the white lines in front of a blind alley. Retracing in the main pathway was not counted as an error. After each trial in the maze, each experimental S was given an ECS consisting of 450 volts for 0.2 sec. through ear-clip electrodes. The amount of current (17–21 ma.) was sufficient to produce grand mal convulsions. Ss in Group I (8 S_1's and 9 S_3's) received an ECS 45 sec. after entry into the goal box. This alowed 30 sec. of feeding and 15 sec. for attachment of the electrodes. Ss in the other 5 experimental groups were also given 30 sec. of feeding after each trial but were given daily ECS treatments at the following time intervals following each goal box entry: Group II (11 S_1's and 13 S_3's), 1 min. and 15 sec.; Group III (7 S_1's and 7 S_3's), 1 min. and 45 sec.; Group IV (11 S_1's and 16 S_3's), 5 min. and 15 sec.; Group V (8 S_1's and 10 S_3's), 30 min.; Group VI (11 S_1's and 11 S_3's), 2 hrs. Ss in Group VII, the control group (9 S_1's and 9 S_3's), were not convulsed, but had electrodes attached to their ears for 2 sec., 45 seconds after each goal box entry.

Results and Discussion

The mean number of errors made by S_1 rats and S_3 rats in each of the 7 groups is shown in Figure 1. As can be seen, in Groups I through V

(trial-ECS interval from 45 seconds to 30 minutes) the means of the S_1 animals are lower than those of the S_3's. In Group VI (2-hour interval) the S_3 rats are superior ($t = 2.36$ df $= 20$, $P < .025$) to S_1 rats and in Group VII (no ECS) the means for the two strains are almost identical ($S_1 = 16.74$; $S_3 = 15.35$).

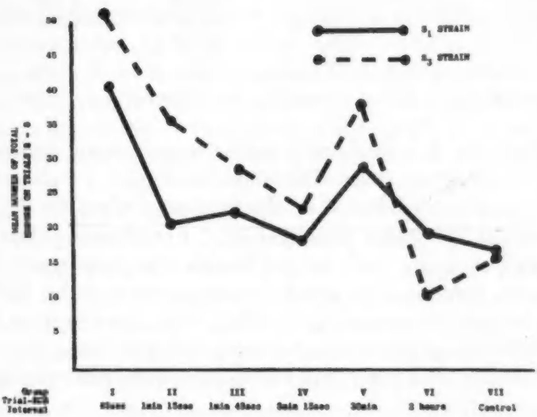

FIGURE 1. Mean number of total errors made by S_1 and S_3 rats in each of the groups. Times indicate the time interval between goal box entry and ECS treatment for the six experimental groups.

An analysis of variance of these data computed according to a procedure allowing for unequal subclass frequencies (Walker and Lev, 1953, pp. 381–2) is shown in Table I.[2] The F values for ECS and strain are both statistically significant, but the F for interaction failed to reach significance.

The finding of a significant impairing effect of post-trial administrations of ECS is consistent with the findings of several investigators (for example, Duncan, 1949; Leukel, 1957; Thompson & Dean, 1955). The results of this study thus provide additional support for our hypothesis (which is a reformulation of the Müller and Pilzecker Perseveration-Consolidation Hypothesis, 1900) which assumes that the neural processes underlying learning become consolidated, or fixed, as a consequence of uninterrupted perseveration of the neural activity initiated by a training trial.

[2]The hypothesis of homogeneity of variance was rejected by a Hartley test (Walker and Lev, 1953, pp. 192–3). However, in view of the tested robustness of the F test, no scale transformations were used in this analysis.

TABLE I

ANALYSIS OF VARIANCE OF TOTAL ERRORS MADE ON TRIALS 2–8 IN THE LASHLEY III MAZE

Source of Variation	df	MS	*F*
ECS treatment	6	239.09	12.68**
Strain	1	93.03	4.94*
Interaction	6	24.09	1.28
Within	126	18.85	
Total	139		

**Significant at .005 level. *Significant at .05 level.

More important for the hypothesis under investigation, however, is the finding of a significant strain difference. As Figure 1 indicates, the S_3 rats were more severely affected by the treatment when the ECS was administered within 30 minutes after each trial. For Groups I through V, the mean number of errors made by the S_3 rats was consistently higher than that of the S_1 animals. One possible interpretation of this finding is that the ECS produced brain damage in the S_3 rats. The results of Group VI argue against this interpretation, however, since in this group which received ECS 2 hours after each trial, the S_3 rats made fewer errors than the S_1 animals.

As Table I indicates, the strain by ECS interaction was not significant. However, this should not be considered as evidence of a lack of a differential effect of ECS on the two strains. In Groups VI and VII the means of the S_3 rats were either slightly lower than or equal to those of the S_1 animals, while in all other groups the S_1 rats made fewer errors. Thus S_1 superiority in learning is found only with the shorter (45 second–30 minute) trial-ECS intervals. An inspection of Figure 1 suggests that, with the exception of Group V (30-minute ECS), the magnitude of the ECS effect decreases with increasing trial-ECS intervals. A detailed examination of the records offered no explanation for the Group V results.

The finding of a differential effect of ECS on animals of the two strains supports our general hypothesis that the basis of the strain differences in learning typically obtained may be due to a difference in consolidation rate. When time for consolidation is allowed (Group VII), the two strains do not differ in learning ability. The S_1 rats make fewer errors than the S_3 only when trials are massed, or when ECS is administered after each trial. These findings, taken together, provide support for the hypothesis that the rats from the S_1 strain consolidate more rapidly than those from the S_3 strain.

A possible basis for the hypothesized strain difference in consolidation rate is suggested by the findings of Rosenzweig, Krech, and Bennett (1960) that rats from the S_1 strain have higher levels of cholinesterase (ChE) activity and greater concentrations of acetylcholine (ACh) than those from the S_3 strain. Since there is evidence that these substances are involved in neural transmission in the central nervous system (Crossland, 1960) strain differences in amounts or activity of these substances might produce differences in rate of neural reverberation following each learning trial. If it is further assumed that consolidation following a learning trial results from reverberating neural circuits (Hebb, 1949), then strain differences in consolidation rate might be due to a difference in reverberation *rate* resulting from strain differences in ACh and ChE levels.

Further evidence of a difference in CNS activity in rats of the two strains is provided by the recent finding (Woolley, Rosenzweig, Krech, Bennett, and Timiras, 1960) that the S_1 animals have lower ECS thresholds (minimal seizure) than the S_3 ones. This finding suggests that the results of the present study cannot be explained in terms of a strain difference in ECS thresholds, since the learning of the S_3 rats was more seriously affected by the post-trial ECS treatments.

Since the two strains investigated in the present study no doubt differ in many respects (for example, neural structural and/or biochemical differences other than ACh and ChE levels), it is of course possible that the findings of the present study were mediated by some other mechanism(s). Additional support for the present hypothesis would be provided if, in future studies using other strains, ECS effects on learning could be predicted from a knowledge of ACh and ChE levels. With the development of new strains of rats with known levels of ACh and ChE (Rosenzweig, Krech, and Bennett, 1960) such studies should soon be feasible.

Summary

Sixty-five S_1 Ss (Tryon maze-brights) and 75 S_3 Ss (Tryon maze-dulls) were given 1 trial a day for 8 days in a Lashley III alley maze. Ss in 6 experimental groups were given an ECS at different time intervals following each trial (45 seconds; 1 minute, 15 seconds; 1 minute, 45 seconds; 5 minutes, 15 seconds; 30 minutes; and 2 hours). A control group was not shocked. An analysis of variance indicated significant ECS effects and strain differences. When ECS was administered within 30 minutes, the S_3 Ss made more errors than the S_1 Ss. The mean number of errors made by S_1 and S_3 Ss did not differ significantly, however, in the control group. It was suggested that the strain differences in ECS effects were due to strain differences in post-trial consolidation and that the differences in consolidation rate might be mediated by strain differences in level of acetylcholine and cholinesterase activity.

REFERENCES

BREEN, R. A., & MCGAUGH, J. L. Facilitation of maze-learning with post-trial injections of picrotoxin. *J. comp. physiol. Psychol.* (in press).

CROSSLAND, J. Chemical transmission in the central nervous system. *J. Pharmacy & Pharmacol.*, 1960, **12**, 1–36.

DUNCAN, C. P. The retroactive effects of electroshock on learning. *J. comp. physiol. Psychol.*, 1949, **42**, 32–4.

HEBB, D. O. *The organization of behavior*. New York: Wiley, 1949.

JENNINGS, R. D. Strain differences in the effect of distribution of practice on maze learning. Unpublished Master's Thesis, San Jose State College, 1960.

LEUKEL, F. A comparison of the effects of ECS and anesthesia on acquisition of the maze habit. *J. comp. physiol. Psychol.*, 1957, **50**, 300–6.

MCGAUGH, J. L. Neurochemical regulation of learning. Unpublished paper presented at the Western Psychological Association Meeting, San Jose, April, 1960.

MCGAUGH, J. L., WESTBROOK, W. H., & BURT, G. S. Strain differences in the facilitative effects of 5-7-diphenyl-1-3-diazadamantan-6-01 (1757 I.S.) *J. comp. physiol. Psychol.* (in press).

MCGAUGH, J. L., WESTBROOK, W. H., & ROSS, R. B. Retroactive effects of 5-7-diphenyl-1-3-diazadamantan-6-01 (1757 I.S.) on maze learning and retention. Unpublished paper presented at the Western Psychological Association Meeting, San Jose, April, 1960.

MÜLLER, G. E., & PILZECKER, A. Experimentelle Beiträge zur Lehre vom Gedächtniss. *Z. Psychol.*, Ergbd. 1900, **1**, 1–288.

ROSENZWEIG, M. R., KRECH, D., & BENNETT, E. L. A search for relations between brain chemistry and behavior. *Psychol. Bull.*, 1960, **57**, 476–92.

ROWLAND, G. L., & WOODS, P. J. Performance of the Tryon bright and dull strains under two conditions in a multiple T-maze. *Canad. J. Psychol.*, 1961, **15**, 20–28.

THOMPSON, R., & DEAN, W. A. Further study on the retroactive effect of ECS. *J. comp. physiol. Psychol.*, 1955, **48**, 488–91.

WALKER, H. M., & LEV, J. *Statistical inference*. New York: Henry Holt, 1953.

WOOLLEY, D. E., ROSENZWEIG, M. R., KRECH, D., BENNETT, E. L., & TIMIRAS, P. S. Strain and sex differences in threshold and pattern of electroshock convulsion in rats. *The Physiologist*, 1960, **3**, 182.

(*Received December 7, 1960*)

SOME BEHAVIOUR CHARACTERISTICS OF MAZE-BRIGHT AND MAZE-DULL ANIMALS[1]

JANE STEWART[2]
McGill University

THE CLOSED FIELD MAZE (Hebb & Williams, 1946; Rabinovitch & Rosvold, 1951) was designed to measure a rat's ability to solve new and varied maze problems. The distribution of performance scores on the maze is such that one can clearly separate maze-bright from maze-dull animals. The maze has been used for screening in selective breeding experiments (Thompson & Bindra, 1952) and for measuring "intelligence" in animals (Cooper & Zubek, 1958; Thompson & Heron, 1954a; Woods, 1959). However, little is known about the factors that constitute intelligence in this sense.

The closed field maze consists of a box 30 inches square and 4 inches high, with an entrance at one corner and a food compartment in the corner diagonally opposite. Fourteen separate barriers of lengths varying from 5 to 25 inches make it possible to set up any one of a series of problems. A new problem is set up for each test session. A performance score on this maze is an error score; it is made up of the total number of maze units which an animal enters and re-enters over a series of problems before it reaches the goal box. A low score indicates maze-brightness.

In an attempt to discover the behaviour characteristics which determine high and low error scores, a series of simple tests was administered both to a group of maze-bright and to a group of maze-dull rats. The tests were designed to provide an opportunity to observe the way in which these animals became habituated to a new situation, approached new problems, eliminated blind alleys, and found their way to food. The test yielded measures of locomotion in a maze, habit reversal learning, black-white discrimination, multiple-choice performance, and emotionality.

METHOD

Two groups of 7 animals each had been selected from 114 male hooded rats for either superior or inferior performance on the closed field maze.[3] The total scores for these animals on 11 test and 11 re-test problems can be found in Table I. It may be seen that the scores of the two groups are clearly separated.

[1]This research was supported by a United States Public Health grant (No. M-2455) to Dr. D. O. Hebb. The author is grateful to Dr. Hebb for his helpful suggestions during the preparation of the manuscript.

[2]Now at Ayerst Research Laboratories, Box 6115, Montreal, Canada.

[3]The selection of animals was carried out by Mr. John Kenyon.

At the time of the tests the animals were approximately 120 days old. Throughout the series of tests the animals were maintained on a 23-hr. food deprivation schedule. The tests were administered in the order in which they are listed below.

TABLE I

TOTAL ERROR SCORES ON THE CLOSED FIELD TEST OF THE RATS SELECTED AS SUBJECTS

Maze-Bright		Maze-Dull	
1	141	1	297
2	143	2	301
3	155	3	305
4	156	4	322
5	157	5	324
6	158	6	327
7	159	7	328

Locomotory Behaviour

For this test an enclosed Y-maze was employed, similar to that used by Montgomery (1953) in his tests of exploratory behaviour. The maze had three equal arms placed at equal angles. Each arm was 24 in. long and 4 in. wide, and was made of unpainted wood with a wire mesh top. There was a door at the end of one of the arms through which the animals could be placed into the maze. The floor was marked off into nine units, three in each arm.

On each of 6 consecutive days, approximately 1 hr. after the feeding period, each animal was placed in the apparatus through the door and observed for a 10-min. period. The number of maze units which the animal entered and re-entered in the test period constituted its locomotion score.

Habit Reversal

The same apparatus was employed in this test as had been employed in the test of locomotory behaviour. A food cup was placed at the end of each of the two arms of the maze farthest from the arm with the entry door. Each day one of the cups was filled with wet mash.

The animals were tested approximately ½ hr. before the regular feeding time. Each animal was placed in the apparatus through the entry door. Five sec. after it reached the food, the animal was removed and given another identical trial. On the preliminary test day, food was placed on the left and each animal was given 13 consecutive massed trials. On the following 6 days the animals were given a series of massed trials each day until they reached the criterion of 5 consecutive direct runs to the food cup. The food was placed in the left cup on the 1st, 3rd, and 5th days, and in the right cup on the alternate days. The score was the number of trials required to reach criterion each day.

Black-White Discrimination

The Y-maze described above was also employed in this test. The walls of one of the arms farthest from the arm with the entry door were lined with white cardboard and the walls of the other were lined with black cardboard. The two arms were removable and could be interchanged from trial to trial. Food was always placed in the cup in the white arm.

The white arm with the food was placed on either the right or the left in a random manner from trial to trial. The animals had to learn to make 5 consecutive direct runs to the white arm. The animal was placed in the maze through the door and removed 5 sec. after it reached the food. Each animal was given a series of massed trials; if it had not reached the criterion after 56 trials on the first test day, testing was terminated until the following day. The response measure was the total number of trials to reach the criterion of 5 consecutive direct runs to the white arm.

Multiple-Choice Problem

In this situation the animal had to search for food on each trial. The position of the food was varied at random, affording the animals no fixed solution. The apparatus consisted of a box 24 in. × 6 in. × 6 in. which was divided into four equal compartments. The entrance to each compartment was a rounded arch, 3 in. wide. The face of the box presented to the animal consisted of a 27 in. × 6 in. wall with four identical, equally-spaced entrances. The box was placed on a table and barriers enclosed an area in front of it.

Food was placed in one of the four compartments on each trial according to the following schedule: ABCDACDBCDBABCDACDBACADBBDCAACDB. Each animal was given 32 trials; 16 on each of 2 days. On each trial the animal was placed directly in front of the four doors at a distance of approximately 18 in. Five sec. after the animal found the correct compartment it was removed from the apparatus, the food dish was moved to another compartment, and the animal was given another similar trial. The number and the sequence of compartments entered on each trial were recorded.

Open Field Test of Emotionality

A large open wooden box, 48 in. × 48 in. and 12 in. high, was used for this test. The floor of the box was covered with white oilcloth which was marked off into 25 9.6-in. squares. A large fluorescent light hung over the box.

Approximately 1 hr. after the regular feeding period on 3 consecutive days, each animal was placed in one corner of the box and observed for a 5-min. period. The measures taken were the total number of squares entered, the total number of inside squares entered, and the number of boluses defecated.

Results

The results of the five tests are summarized in Table II. In each case the Mann-Whitney U-test (two-tailed) was used to determine the significance of the difference between the two groups.

Locomotory behaviour. The maze-bright animals consistently entered fewer maze units than the maze-dull. The difference between the two groups was significant on all but the first day of testing.

Habit reversal. The maze-bright animals required significantly fewer trials to reach criterion than the maze-dull on the first three days of this test. However, the differences between the two groups were not significant on the last three days of the test.

Black-white discrimination. The mean number of trials to criterion was 50.71 for the brights and 47.43 for the dulls. This difference was not significant.

TABLE II

MEAN SCORES FOR MAZE-BRIGHT AND MAZE-DULL ANIMALS ON THE FIVE TESTS

Test	Maze-Bright	Maze-Dull	p
Locomotion (no. units entered)			
Day 1	94.71	118.14	.08 to .10
2	63.14	92.71	.04
3	55.86	96.71	.03
4	57.00	100.14	.006 to .009
5	62.00	96.86	.06
6	37.29	71.14	.006 to .009
Habit Reversal (trials to criterion)			
Day 1	10.14	31.86	.002
2	11.43	20.71	.019 to .027
3	8.43	20.86	.006 to .009
4	8.43	8.71	n.s.
5	8.71	11.71	n.s.
6	7.43	11.43	n.s.
Discrimination (trials to criterion)	50.71	47.43	n.s.
Multiple Choice			
Compart. entered	89.71	105.14	.006
Re-entries	13.00	20.71	.05
Open Field			
Total squares	106.87	163.71	n.s.
Inside squares	1.43	10.14	.01
Boluses	13.57	7.29	.06

Multiple-choice problem. The maze-dull animals entered significantly more compartments in the 32 trials than did the maze-bright. A further difference was found by counting the number of re-entries into a wrong compartment made on any one trial. The mean number of re-entries on 32 trials made by the two groups was: maze-bright, 13.00; maze-dull, 20.71 ($p < .05$).

Open field test. Over the three test sessions the total number of squares entered by the maze-bright animals was smaller than that entered by the maze-dull. This difference did not reach an acceptable level of significance, but it is in the same direction as the difference found in the first test. The brights did, however, enter significantly fewer inside squares and defecated more boluses than the dulls.

DISCUSSION

The results of the present experiments show that dull animals entered and re-entered more squares in the Y-maze, made a greater number of errors in habit reversal learning, entered and re-entered more compartments in the multiple-choice problem, and tended to enter more squares

in the open field test. In each of these tests, where free movement was allowed, the dull animals showed greater locomotory activity. Only the black-white discrimination test failed to differentiate between the two groups. This lack of difference might be due to the fact that the discrimination test was run immediately after, and in the same apparatus as, the test of habit reversal. By the time the discrimination test was initiated the animals had had 12 sessions in the apparatus.

A number of other investigators have also noted that animals which make high error scores in mazes, as a consequence of brain damage (Thomas, Moore, Harvey & Hunt, 1959) or early restriction (Thompson & Heron, 1954a; Thompson & Heron, 1954b; Woods, 1959; Zimbardo & Montgomery, 1957), also obtain high scores in measures of general activity and locomotory behaviour.

Woods (1959) and Woods, Ruckelshaus, and Bowling (1960) have discussed this relation in two recent papers in which they reported on the effects of free and restricted environmental experience on scores in the closed field maze. They now have considerable evidence that "restricted" animals which make the most errors in the closed field maze are those animals which "explore" the most in an independent test situation. It should be noted here that these authors used a locomotion score as a measure of exploration. They conclude that their "results as a whole indicate that exploratory differences and not intelligence differences seem to be the major factor in the characteristic finding that Ss reared in a 'free' environment are superior problem-solvers when compared with Ss reared under restricted conditions." Their conclusion is supported by the fact that in a discrimination problem, where free movement was minimized, the restricted and free animals did not differ in learning ability.

The above considerations raise the question of the relation between persistent locomotory behaviour, as observed in the present experiments, and maze performance. Do the dull animals make more errors (enter more blind alleys) because they make more locomotory responses which interfere with the task at hand or is their excessive locomotory behaviour merely a sign of their inability to learn?

Summary

Two groups of 7 animals each, which had been selected from 114 male hooded rats for either superior or inferior performance on the closed field maze were tested on a series of simple tests in an attempt to discover the characteristics of behaviour which determine high and low error scores in this maze. The tests yielded measures of locomotion, habit reversal learning, black-white discrimination, multiple-choice performance, and emotionality.

The results show that dull animals made more locomotion responses, a greater number of errors in habit reversal learning, entered and re-entered the wrong

compartment more frequently in the multiple-choice problem, and tended to enter more squares in the test of emotionality. The question of the relation between persistent locomotory behaviour and "intelligence" is discussed.

REFERENCES

Cooper, R. M., & Zubek, J. P. Effects of enriched and restricted early environments on the learning ability of bright and dull rats. *Can. J. Psychol.*, 1958, **12**, 159–64.

Hebb, D. O., & Williams, K. A method of rating animal intelligence. *J. genet. Psychol.*, 1946, **34**, 59–65.

Montgomery, K. C. The effect of the hunger and thirst drives upon exploratory behavior. *J. comp. physiol. Psychol.*, 1953, **46**, 315–19.

Rabinovitch, M. S., & Rosvold, H. E. A closed field intelligence test for rats. *Can. J. Psychol.*, 1951, **5**, 122–8.

Thomas, G. J., Moore, R. Y., Harvey, J. A., & Hunt, H. F. Relations between the behavioral syndrome produced by lesions in the septal region of the forebrain and maze learning of the rat. *J. comp. physiol. Psychol.*, 1959, **52**, 527–32.

Thompson, W. R., & Bindra, D. Motivational and emotional characteristics of "bright" and "dull" rats. *Can. J. Psychol.*, 1952, **6**, 116–22.

Thompson, W. R., & Heron, W. Effects of restriction early in life on problem-solving ability in dogs. *Can. J. Psychol.*, 1954a, **8**, 17–31.

—— The effects of early restriction on activity in dogs. *J. comp. physiol. Psychol.*, 1954b, **47**, 77–82.

Woods, P. J. The effect of free and restricted environmental experience on problem-solving behavior in the rat. *J. comp. physiol. Psychol.*, 1959, **52**, 399–402.

Woods, P. J., Ruckelshaus, S. I., & Bowling, D. M. Some effects of "free" and "restricted" environmental rearing conditions upon adult behavior in the rat. *Psychol. Rep.*, 1960, **6**, 191–200.

Zimbardo, P. G., & Montgomery, K. C. Effects of "free environment" rearing upon exploratory behavior. *Psychol. Rep.*, 1957, **3**, 589–94.

(*Received June 23, 1960*)

THE EFFECT OF HUNGER SATIATION IN THE TEST PHASE ON INFERENCE PERFORMANCE[1]

KAREL LAMBERT AND CHARLES N. UHL
University of Alberta

A RECENT STUDY BY LAMBERT (1960) found evidence for latent inference learning in rats, satiated for food during the training (latent) phase, but unsatiated during the test phase. The technique for satiating the rats in that study did not preclude the possibility that, in the training phase, the rats might have been hungry, but not hungry enough to eat. If so, then it might well have been the case that the reaction potential in favour of the correct response at the end of the training phase was high enough to evoke that response in the test phase without the rats having been made hungry enough to eat.[2] The obvious test of this hypothesis is to repeat the Lambert experiment under the same conditions, but to eliminate the deprivation regimen prior to the test phase. The present study establishes the falsity of the hypothesis in question. In so doing, it supports the belief that, in the Lambert set-up, a hunger drive strong enough to produce eating is a *necessary condition* for latent inference performance.

METHOD

Subjects

Ss were 38 naïve, virgin, male, Wistar albino rats about five months old. They were purchased from the Northwest Rodent Farm, a local laboratory animal supplier.

Apparatus

The apparatus was a replica of the Tolman-Gleitman (1949) lazy E-alley-maze, with distinctive and detachable end-arms and end-boxes. The precise character of the apparatus is described by Lambert (1960).

Procedure

Since the procedure in the present study, except for the test stage, is the same as that fully described in Lambert (1960), only a sketch is presented here. In Stage I, all Ss were given four forced trials per day for ten days in the full maze, while satiated for food (as in Lambert 1960) and with no food present in either end-box. The runs were so arranged that each S received equal experience with each end-arm and end-box on any given day. In Stage II, the end-arms were removed from the maze and Ss, still satiated, were given four trials per day (for two days) in each end-arm and end-box. During this stage, food was continually present in only one end-box: this was the black end-box for half the Ss (group A) and the white end-box for the other half (group B).

---

[1]This study was supported by a grant from the General Research Fund of the University of Alberta.

[2]The reasoning behind this hypothesis has been presented in Lambert (1960) on page 49.

Test Stage

The maze was restored as in Stage I and all food removed. Ss, satiated for food as in the training series, were now placed in the maze. With all doors open, Ss were given one free run in the maze.

Results and Discussion

The results on the test trial show that out of the 19 rats in group B (food in the white end-box), 8 chose correctly; and out of the 19 in group A (food in the black end-box), 10 chose correctly. These results clearly indicate no significant departure from a chance expectation of 50 per cent correct response, and consequently do not support the hypothesis that the reaction potential in favour of the correct response at the end of the training phase in the Lambert set-up is high enough to evoke that response in the test phase when the rat is not hungry enough to eat. Apparently, then, to obtain latent inference performance under the Lambert conditions, it is necessary that the hunger drive level be high enough to produce eating behaviour.[3]

Summary

Rats, satiated for food, were given equal experience in both halves of the Tolman-Gleitman lazy E-alley-maze with no food present in either end-box. Then, still operationally satiated for food, they experienced food in one end-box and nothing in the other end-box, the end-boxes now being detached from the maze. Finally, in the restored maze, and still operationally satiated for food, they were given one free run with all doors open. Out of 38 animals, 18 ran to the side where they had formerly found food.

REFERENCES

Lambert, Karel. A study of latent inference. *Canad. J. Psychol.*, 1960, **14**, 45–50.

Tolman, E. C., & Gleitman, H. Studies in learning and motivation. I. Equal reinforcements in both end-boxes followed by shock in one end-box. *J. Exp. Psychol.*, 1949, **39**, 810–19.

[3]Twenty-four hours after the satiation test all animals were given another day of training in the E-maze under the conditions prevailing in Stage II of the training series. They were then placed in the maze after 32–34 hours without food and were given one free run. Out of the 19 animals in group B, 14 chose correctly, while 15 of the 19 animals in group A chose correctly. Applying the binomial test as in Lambert (1960), the probability of obtaining 29 correct choices out of 38 by chance is less than .01 (one tail). Though not, in the strict sense, statistically comparable, the different behaviour of the animals in the hunger test situation tends to reinforce the conclusion mentioned above in the discussion section of this report.

(Received October 14, 1960)

PERCEPTUAL CHANGES AFTER PROLONGED SENSORY ISOLATION (DARKNESS AND SILENCE)[1]

JOHN P. ZUBEK, DOLORES PUSHKAR, WILMA SANSOM, AND J. GOWING
University of Manitoba

IN AN EARLIER REPORT (Zubek, Sansom, & Prysiazniuk, 1960) a description was given of the intellectual changes occurring in sixteen subjects during a week of isolation under conditions of constant darkness and silence. In the present paper the concern is largely with the perceptual changes recorded in these same subjects.

METHOD

The experimental conditions were similar to those previously described (Zubek, Sansom, & Prysiazniuk, 1960). Briefly, they were as follows: the Ss were paid to lie on an air mattress in a dark and soundproofed chamber (70 db. attenuation). They constantly wore a pair of earmuffs which served to reduce any sounds that they might make inside the chamber. They were instructed to lie quietly on the mattress and not to engage in any singing or humming or any other vocal or physical activity. Toilet facilities, a food chamber, a panic button, a two-way intercom system, and an air-conditioning unit are all built into the floor making it unnecessary for S to leave the chamber for any purpose during the isolation period. The Ss were asked to remain in isolation for a week and during this time were prevented as far as possible from determining what time it was. The only intrusion on the experimental condition of darkness and silence was an occasional test session, for purposes of appraising intellectual abilities, of approximately 45 minutes duration when a 15-watt red bulb was put on inside the chamber. Since the results of these intellectual tests have already been reported in an earlier publication, they will not be dealt with here.

The experimental group consisted of 16 Ss (12 males and 4 females). Of these, 14 were in isolation for 7 days, one for 8½ days, and one for 10 days. A control group of Ss was also employed. Further details about these Ss have been given in the earlier publication. The experimental Ss were given a battery of perceptual-motor tests before and immediately after the isolation period. These were all administered outside of the isolation chamber under a constant level of illumination. The control Ss were given the same perceptual tests at the same time intervals as the experimentals.

The battery of perceptual-motor tests consisted of the following.

(1) *Visual vigilance* was measured by a modified Mackworth clock test. It consisted of an electric laboratory clock (8 in. in diameter) with a single rotating hand which was briefly stopped (0.10 secs.) and then started at eight irregular time

[1]This research was supported by the Defence Research Board of Canada, Project no. 9425-08. The authors wish to express their gratitude to the Children's Hospital, Winnipeg, for making available the facilities of their EEG laboratory, and to Dr. M. G. Saunders of the General Hospital and of the Department of Physiology, University of Manitoba, for his analysis of the EEG records.

intervals during each 30-min. period. The "breaks" or signals were presented at intervals of 2, 2, 3, 1, 6, 7, 3, and 5 min. This 30-min. cycle was repeated four times. S indicated the presence of a signal by pressing a button.

(2) *Auditory discrimination.* While S was performing the vigilance task he was presented with a continuous tone of 1100 cycles at 25 db whose frequency and intensity were periodically increased or decreased in magnitude. Six frequency (50 cycles) and six intensity (5 db) changes were made in each of six, 20-min. periods. The 12 changes or signals were presented at random time intervals in each of the six time periods. S again indicated the presence of a signal by pressing a button.

(3) *Depth perception* was tetsed with the Howard-Dohlman apparatus. Each test of depth perception was the mean of four binocular trials where half of the trials started with the movable rod in front of the standard and the other half with it in the back. The S, whose head was kept immobilized, was at a distance of 12 ft. from the apparatus. Scores are recorded in terms of the mean separation, in cm., of the variable rod from the standard (taken as zero).

(4) *Size constancy.* A black equilateral triangle, of variable height, was presented at 15 ft. S was required to adjust the height of this triangle until it looked the same in size as the near one, the standard triangle. This triangle was 20 cm. high and was presented at a distance of 4 ft. Four trials were given.

(5) *Reversible figures.* S was asked to fixate a point at the centre of an ambiguous figure (reversible blocks) for 60 sec., pressing a counter each time the figure changed. Three trials were given. The score was the average number of reversals per minute.

(6) *Perception of lines.* S was shown a horizontal black line, 10 in. long and ¼ in. wide with a fixation point located 3 in. above and below the centre of the line. He was then shown two parallel vertical black lines, each 10 in. long and ⅜ in. wide, separated by 2⅜ in. with a fixation point located between the two parallel lines. These two sets of lines were presented on a white background at a distance of 3 ft. from S who was instructed to "tell me what each figure looks like to you—not what you think it really is, but what it looks like subjectively."

(7) *Perception of colours.* S was shown 6 coloured, 5-in. squares of paper—red, green, yellow, blue, black and white—and asked to report on any changes in brightness or richness in colour.

In addition to these perceptual tests which were given before and after isolation, or a week apart in the case of the controls, the experimental Ss were required to estimate various intervals of time before and at daily intervals during isolation. Starting at the sound of a buzzer the Ss were asked to signal when 1, 3, 5, 15, 30, 60, and 120 min. had elapsed. All time estimates were from zero, that is, the Ss making a 5-min. estimate, for example, were told to signal when they felt a total period of 5 min. had elapsed rather than the passage of a 2-min. period after the 3-min. estimate. During all of the time estimates the Ss were told to refrain from any intellectual activity and to keep their minds as blank as possible. In several instances the Ss fell asleep during the test. When this occurred the time estimates were repeated a little later.

The ideal time control Ss for this study would have been the experimentals, retested some months after isolation. However, since many of them had left the city only eight could be used as controls. They were all tested from 4 to 6 months after isolation. The rest of the controls were made up of Ss as close as possible in age, education, intelligence, and so forth, to the experimentals. Most of the time estimate controls were tested in the isolation chamber. The remainder were tested in a quiet

room wearing black goggles and earmuffs. At the completion of the daily time estimates the Ss were kept in the laboratory for a period of time before being allowed to go home. This was done in order to prevent knowledge of results unduly influencing the subsequent daily time estimates.

Results

Perceptual Tests

Table I shows the results of the experimental and control subjects on the tests of depth perception, size constancy, and reversible figures administered before and after a week of isolation or an elapse of a week.

TABLE I

Mean Scores on Depth Perception, Size Constancy, and Reversible Figures

	Experimentals	Controls
Depth perception		
N	16	16
Before	2.00 cm.	1.97 cm.
After	2.83 cm.	1.56 cm.
Size constancy		
N	15	15
Before	21.9 cm.	20.3 cm.
After	23.9 cm.	20.5 cm.
Reversible figures		
N	15	15
Before	15.8/min.	15.3/min.
After	13.4/min.	15.2/min.

The statistical analysis is based on the difference between the mean scores of the experimental and control groups after a week, relative to their scores a week earlier. It can be seen that on depth perception the experimental subjects did less well after a week than did the controls. This difference, however, is not reliable ($.05 < p < .10$). On the size constancy test, the experimentals chose larger comparison triangles than did the controls, suggesting a decrease in constancy, but this difference again is not statistically reliable ($.05 < p < .10$). However, since these two sets of results border on significance it is possible that with a larger N they might have reached statistical significance. Finally, on the reversible figures the experimentals showed fewer figural reversals per minute than the controls but this difference again is not reliable ($p > .40$).

Figure 1 shows the performance of the experimental and control subjects on the auditory discrimination task in terms of percentage detection of signal changes, frequency and intensity combined, occurring in six suc-

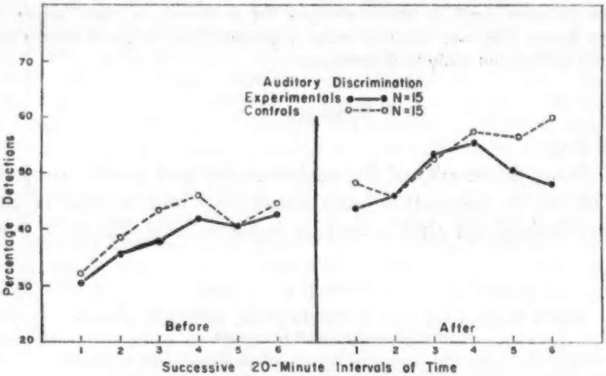

FIGURE 1. Performance of experimental and control subjects on an auditory discrimination task in terms of percentage detection of signal changes occurring in six successive 20-minute intervals of time.

cessive 20-minute intervals of time. A statistical analysis based on a comparison of the mean difference scores (between the pre-isolation and week-later tests) of the two groups shows no reliable difference in performance on the entire 120-minute test ($p > .10$) nor on any of the six 20-minute test periods, although the difference on the sixth 20-minute period borders on significance ($.05 < p < .10$). Separate curves for frequency and intensity changes are not shown because no reliable differences were evident between the two measurements.

Figure 2 shows the scores of the experimental and control subjects on the visual vigilance task in terms of percentage detection of signal changes occurring in four successive 30-minute periods. It can be seen that, after a week, the over-all vigilance performance of the experimentals is much poorer than that of the controls ($p < .001$). Furthermore, their performance on each of the four successive 30-minute periods is reliably poorer than that of the controls. It is interesting to note that, although the vigilance performance of the control subjects has levelled off during the last 60 minutes, it is still deteriorating in the experimentals.

In the daily time estimation tests it was found that both the experimental and control subjects overestimated[2] the short intervals of time,

[2]In keeping with the suggestion of Bindra and Waksberg, 1956, underestimation of time by the production method refers to "subjective temporal units smaller than objective temporal units" whereas overestimation refers to "subjective temporal units larger than objective temporal units."

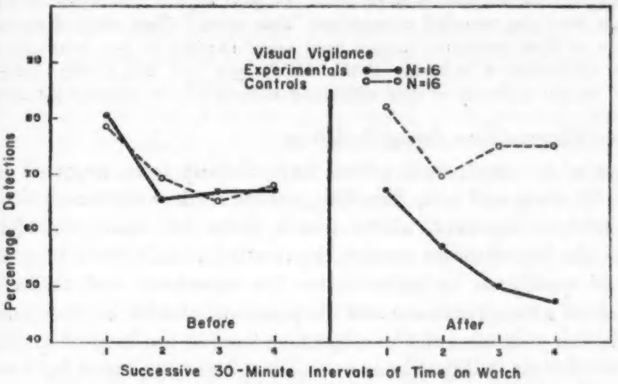

FIGURE 2. Performance of experimental and control subjects on a visual vigilance task in terms of percentage detection of signal changes occurring in four successive 30-minute intervals of time.

that is, 1, 3, 5, 15, and 30 minutes. However, none of the differences between the groups is statistically reliable. On the longer time intervals, that is, 60 and 120 minutes, both groups of subjects showed an underestimation, with the experimentals showing more underestimation, that is, less accuracy than the controls. However, only on the 120-minute estimate is the difference statistically reliable ($.01 < p < .02$).

Some observations of an exploratory nature were also made of the ability of 10 of the 16 subjects to report the time of day. These subjects were asked, at an *average* of once a day, to report the clock time. Their estimates were surprisingly accurate, showing an average error of 105 minutes (S.D. = 76.5) over the seven days. No reliable difference was found between the average error of estimate of the first three days and that of the last four days. However, the error during the first day was significantly greater than that of any of the succeeding days. The general tendency of the errors was in the minus direction, that is, to underestimation. The "accuracy" of these estimates may partly be due to the presence of certain cues, for example from the occasional battery of intellectual tests (given at intervals from 18 to 30 hours) and also to possible vibrations passing into the chamber. Whatever the explanation for this accuracy might be, these results are in agreement with those obtained by MacLeod and Roff (1936) who, in perhaps the first experimental study of isolation, confined two subjects in a sound-proofed room for 86 and 48 hours and asked them at various intervals, to report the clock time. They found that the errors were "surprisingly small." The final error of the 86-hour subject was 40 minutes while that of the 48-hour subject was 26 minutes. Furthermore, they reported an "initial large error of orientation after the first night of sleep, which gradually became reduced and eventually disappeared almost completely." These results are also in line with those of Smith and Lewty (1959) on a group of 20 subjects kept in a silent but constantly illuminated room for periods up to four days. Their error of

estimate ranged from one-half to three hours. Subjects kept in a tank-type respirator for 36 hours have also revealed an excellent "time sense." Their error of estimating the duration of their restriction ranged from seven minutes to two hours (Wexler, Mendelson, Leiderman & Solomon, 1958). Goldberger and Holt (1958) have also commented on the accuracy of time estimation in their 8-hour isolation experiments.

Qualitative Observations during Isolation

Changes of an intellectual nature have already been reported in an earlier publication and will, therefore, not be dealt with here. One type of phenomenon, however, about which there has been considerable interest in the literature on sensory deprivation is hallucinatory activity. In order to qualify as an hallucination the experience had to have an "out-thereness"; its appearance and disappearance had to be independent of the subject's volition, and the subject, at least in the beginning, had to be convinced of its reality. These were the criteria employed by Vernon, McGill, and Schiffman (1958) in their paper on visual hallucinations occurring during three days of darkness and silence. If these criteria are applied to the present study we can say that 11 out of the 16 subjects experienced some hallucinatory activity at some time during the period of isolation. When hallucinations were reported they usually appeared after the third day. The majority of the hallucinations were of the Type I variety, a category employed by Vernon, McGill, and Schiffman (1958) to describe hallucinations characterized by "flashes of light, flickering lights, dim glowing lights, etc., which lack shape and usually appear in the peripheral field of vision." A few Type II hallucinations consisting of geometric shapes (for example, circles, squares, latticework) were reported, but usually towards the end of the isolation period. Only two instances of Type III hallucinations, that is highly structured, integrated scenes of an animated nature, were reported. One of these consisted of a TV screen showing a blurred message which the subject tried to read but was unable to. The other consisted of two moving eyeballs of a "most frightening nature." Both of these were reported during the sixth day.

There was little evidence of increasing hallucinatory complexity with increasing time in isolation. The changes which did take place usually consisted of an increase in the brightness and number of the Type I hallucinations present at one time rather than a change in their quality or a transformation to the Type II variety. The hallucinations usually were of very short duration, about 5 to 10 seconds, although some were reported to last for as long as 15 minutes. Although most of the subjects showed hallucinatory activity of one degree or another, it is important to note that in general this activity took up only a very small portion of the subject's time in isolation. Many subjects reported only one or two

brief hallucinatory periods a day, others only one or two during the entire week and some (5) reported no hallucinatory activity whatsoever.

In addition to visual hallucinations a number of auditory hallucinations were reported. These were usually very realistic in nature, for example, howling dogs, ringing of alarm clock, sound of typewriter, policeman's whistle, and dripping water. Two hallucinations of a tactual-kinesthetic nature were also reported. One consisted of cold steel pressing on the subject's forehead and cheeks and the other a sensation of someone pulling the mattress from under the subject. These auditory and tactual hallucinations, if present, were usually reported during the last two days of isolation.

There were some *suggestions* that female subjects may be less prone to hallucinations than males. Of 12 male subjects, ten had hallucinations, while out of four female subjects only one reported them. Her hallucinations were of the Type I variety and occurred relatively infrequently. Although this sample is too small for any definite conclusions to be drawn, these sex differences are suggestive and should warrant further investigation by workers in the field of sensory deprivation. There are already some suggestions that females can take isolation for longer periods than males (Smith & Lewty, 1959).

In addition to reporting hallucinatory activity, many subjects reported certain changes in imagery. At various intervals during isolation the subjects were asked to imagine or visualize certain familiar scenes, for example, lakes, countryside, the inside of their homes, and so forth. The majority of the subjects reported that the images which they conjured up were of unusual vividness, were usually characterized by bright colours, and had considerable detail. All these subjects were unanimous in their opinion that their images were more vivid than anything they had previously experienced. Several subjects who normally had great difficulty visualizing scenes could now visualize them almost instantly with great vividness. This enhanced imagery can be illustrated by reference to one subject who could visualize faces of former associates of a few years back with almost picture-like clarity, a thing which he was never able to do previously. This phenomenon usually appeared during the second or third day and, in general, became more pronounced with time. This increased vividness of imagery has been reported by many of the workers on isolation (e.g., Bexton, Heron, & Scott, 1954; Freedman & Greenblatt, 1959; Goldberger & Holt, 1958).

Certain emotional changes were evident in many of the subjects. The most common of these was irritability characterized by annoyance with trivial matters and occasionally with the experimenters. The irritability was usually most noticeable during the second and third days. This

emotional change has been reported by many investigators but not by all (Smith & Lewty, 1959). In a number of cases, this irritability was followed by feelings of depression, brooding, and dwelling on imaginary injustices. After the fifth day, a quarter of the subjects (4) reported feelings of contentment, well-being, and, in two cases, of euphoria. This state of well-being is not a *typical* isolation effect. Its presence in this experiment may be a result of the very long isolation period or it may be a reaction brought about by the realization that the week is almost up and that a substantial monetary reward is imminent. In all likelihood, both factors are involved.

An attempt was made to record as many dreams as the subjects could remember. These were quite numerous especially during the first three or four days. They were largely of an anxiety nature whose main theme concerned death—for example, standing on erupting volcano, being surrounded by ferocious Indians, a knife fight with a giant. Another theme, also of an anxiety nature, concerned restricted space—for example, living in a miniature apartment with ceilings three feet high. Very few dreams of a pleasant nature were reported or could be remembered. These dreams are similar in nature to those reported by Smith and Lewty (1959) who confined their subjects in a soundproofed room for periods up to 4 days.

Qualitative Observations after Isolation

Upon emerging from isolation all 16 subjects reported no *gross* perceptual changes of the type reported by Heron, Doane, and Scott (1956). Furthermore, there were no reports of nausea or dizziness although the subjects did stagger somewhat at first: this was to be expected from the nature of the experimental conditions. When the subjects were asked to fixate on the horizontal and vertical lines presented on the wall, 10 out of the 16 subjects reported some changes. These, however, were barely noticeable and were characterized by a *very slight* wavering, thickening, or change in length of the lines. These changes, when present, lasted for only about a minute. On the colour test, one-half of the subjects reported that the coloured papers and the coloured objects in the laboratory were much brighter and more vivid. The white and black objects seemed to stand out particularly. However, in view of the unreliability of the colour test employed not too much reliance can be placed on these results.

Approximately half of the subjects reported a loss of motivation to study or to carry out various activities which they would normally do. In almost all of these cases this condition did not persist for more than one day. Hyperacuity to sounds was also a very common phenomenon,

especially during the first night when even the slightest sounds could be heard. Furthermore, many sounds which normally might be irritating seemed pleasant and in some cases were even considered delightful. The noise of traffic seemed particularly loud and even somewhat startling.

Report on Two Special Cases

Two subjects, one male and one female, were permitted to remain in isolation somewhat longer than the other subjects whose isolation was arbitrarily terminated at the end of a week. In view of certain unusual phenomena which they experienced they will be singled out for special attention.

Subject A (10 days) was a male, 34 years of age. During the first 7 days S reported experiences very similar to those of the majority of the subjects. The first indications of hallucinatory activity appeared in the third day and consisted of a few, small cloud formations and several pulsating lights in the peripheral field of vision. With successive days these Type I hallucinations (largely pulsating lights) became more numerous, brighter, and of longer duration. During the tenth day the pulsating lights, resembling hundreds of car-lights blinking at night, were experienced constantly. They were particularly vivid the first night at home (isolation was terminated at 10 P.M.), so vivid in fact, that S was unable to sleep all night because of their almost blinding nature. There was still some evidence of hallucinatory activity the following afternoon (16 hours later) when S went into the chamber for further intellectual tests—the dim red light under which S took the tests seemed to pulsate for approximately ten minutes after which the sensation disappeared entirely.

In common with most of the other subjects, irritability was very pronounced especially from the third to the fifth days. However, the most memorable emotional change was the development of a mildly euphoric state beginning sometime in the eighth day. It was characterized by a state of serenity, peacefulness, "all's well with the world," total absence of any worries, and extremly good humour. This state was occasionally punctuated by feelings of hilarity and playing of pranks on the experimenters. It was also characterised by an absence of any intellectual activity with S spending hours daydreaming and reliving various past experiences. There was little interest in food or in any activity which might interrupt this "idyllic" state. It persisted for about three days after termination of isolation. It was an emotional state most unlike anything S had ever experienced before.

Upon emerging from isolation S experienced no nausea or dizziness nor any gross perceptual changes involving distortion of shapes and objects nor their movement. There was a slight quivering of horizontal and vertical lines but this persisted for only a few seconds. The only noticeable change was in the brightness and vividness of colours, especially blacks and whites, and the two-dimensional nature of the environment. Furthermore, people did not seem like people, but merely lifeless objects moving about a stage with S acting as a detached observer. This feeling of detachment or of "not belonging" to the outside world was quite noticeable for the next two days. Furthermore during this time S had a strong craving to go back into the chamber and looked for any pretext to do so again. This feeling or craving was particularly strong the following day when S was required to go back into the chamber to take the last battery of tests. It was only with the greatest effort that he

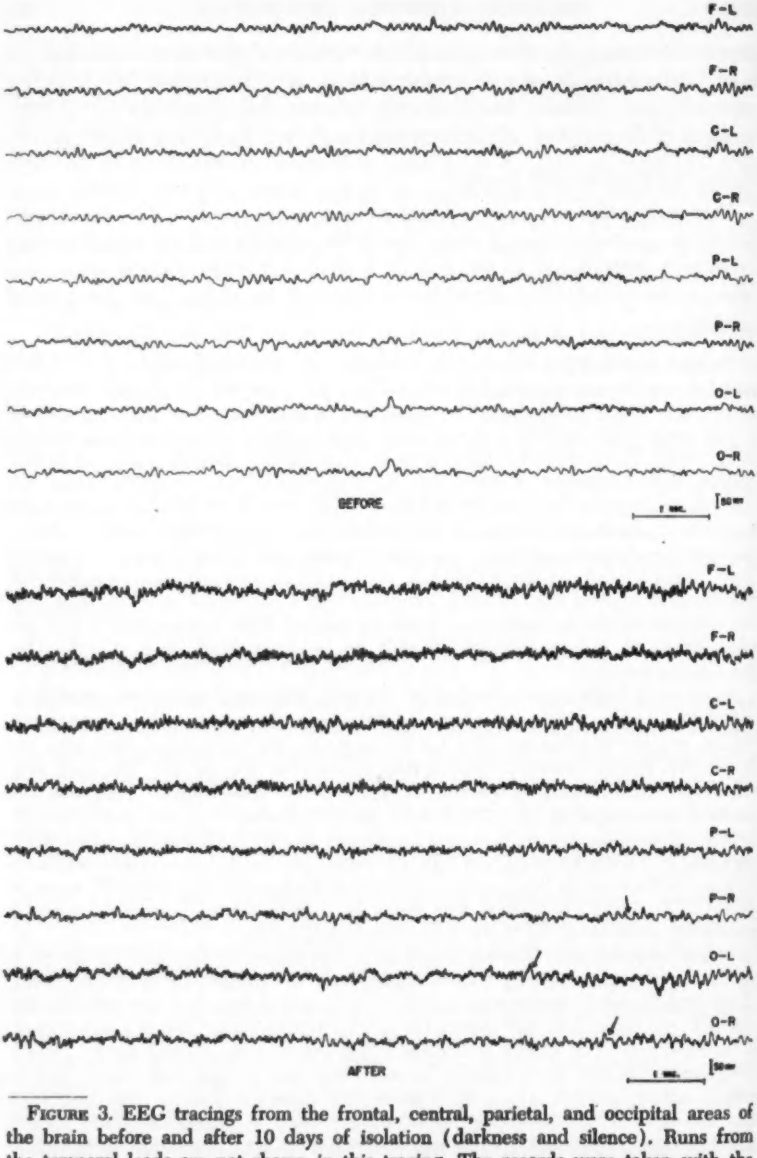

FIGURE 3. EEG tracings from the frontal, central, parietal, and occipital areas of the brain before and after 10 days of isolation (darkness and silence). Runs from the temporal leads are not shown in this tracing. The records were taken with the eyes closed. Note the considerable amount of fast activity in the post-isolation record, particularly evident in the right frontal leads. The arrows indicate some of the scattered slow or theta waves.

could force himself to leave. It would appear as if some form of addiction had developed. This subject was the only one out of the group who reported this craving or even indicated a desire to go back into isolation. It may be related to his long period of isolation or it may be an individual difference.

Another striking and long-lasting experience was a loss of motivation. Although this was present in a noticeable form during the last half of the isolation period it was most obvious in the post-isolation period when the subject, who is the head of a university department, had no desire to do even the most urgent academic duties for a period of at least a week. Furthermore, he showed no interest in delegating some of these duties to his colleagues.

EEG records were taken from this subject before and after the 10 days of isolation. The post-isolation record consisted mainly of fast activity at 12 to 20 c.p.s. (see Figure 3). An excess of slow or theta activity also appeared particularly in runs involving the temporal lobes. In some ways the pattern was reminiscent of a person who had taken barbiturates. During the recording, which took place inside the chamber, S was hallucinating profusely. An attempt was made, therefore, to relate the hallucinatory activity to variations in the EEG record. The only significant factor was a diminution of some of the alpha activity during some of the hallucinations. A repeat EEG taken two weeks later revealed that the pattern had changed markedly. It was now quite normal although some temporal lobe slow activity was still evident but not in excess. This suggested abnormality two weeks later may be related to some of the long lasting post-isolation effects reported by this subject.

EEG records were also taken from 8 Ss before and within 3 hours after emerging from a week of isolation. Two of the Ss showed normal post-isolation records, three showed a slowing of the alpha by one or two cycles and three showed a slowing of the alpha together with an above average amount of slow or theta activity. There was a *suggestion* that this theta activity is somewhat more common in the temporal lobe region. The presence of fast activity in the 10-day S and its absence in the 7-day *Ss* is most puzzling. It may be an individual difference or it may be a change in electrical activity of the brain which occurs only after a week of isolation. These EEG results, together with others which are being accumulated, will be dealt with in some detail in a subsequent publication.

Subject B (8½ days) was a female, 24 years of age. The first appearance of hallucinatory activity occurred, as in subject A, during the morning of the third day and was characterized by a single flickering light in the peripheral field of vision. It seemed to last no longer than a minute and appeared and disappeared several times that morning. No further activity was experienced until the next morning when a number of flashing lights appeared, largely in the left visual field. That evening S reported a most disturbing experience. She imagined that one of the experimenters had told her that her husband was severely injured in a car accident. She then imagined him being carried away in an ambulance, removed to the operating theatre, and then dying in front of her eyes. This experience was so real that she went through all the emotions one would go through if the accident had actually happened. It was only with the greatest effort of restraint that she did not terminate her isolation. Following this experience, no further hallucinatory activity was reported except on the seventh day when several large glowing lights appeared for a few seconds.

In regard to emotional changes, she exhibited some irritability during the third day, followed by feelings of depression and anxiety during the fourth and fifth days brought about, presumably, by the imagined car accident. From the sixth day on and for a day after isolation, S experienced a feeling of "contentment and

serenity of a pleasant sort" similar in nature to that of subject A but lacking his periodic feelings of hilarity.

Upon emerging from isolation, S reported no perceptual distortions although colours, especially reds and blacks, seemed brighter than normal. In keeping with subject A, S reported a strong feeling of detachment in which she felt she really was not a part of the world. This feeling of detachment is similar in many respects to the "break-off" phenomenon reported by pilots at high altitudes (Clark & Graybiel, 1957). This feeling lasted for about two days. S also reported a very disturbing memory impairment while at work two days after release from isolation. It was characterized by an inability to remember various details of her work. This memory loss was so disturbing that she finished the day in a most distraught state. The impairment was no longer present on the following day. In contrast to subject A no craving to go back into the chamber nor any *long lasting* loss of motivation was reported.

Discussion

A direct comparison of these findings with those of other investigators is difficult because of differences in method of confinement, duration of isolation, degree of sensory restriction, types of capacities measured, and so forth. However, some valid comparisons can be made. In this discussion the main emphasis will be placed on studies which are most similar to the present one in terms of procedure and duration.

In a previous publication (Zubek, Sansom, & Prysiazniuk, 1960) it was reported that prolonged sensory isolation (darkness and silence) had little or no effect on *most* of the intellectual abilities which were appraised. The foregoing results indicate that a similar situation exists with respect to perceptual-motor tasks. There was, for example, no significant impairment of depth perception, size constancy, reversible figures, and auditory discrimination. However, on tasks requiring close visual attention and precision the impairment was considerable. This is best illustrated by the results on the visual vigilance task where the subject has to pay close attention to a moving hand which, at irregular intervals, stops for a fraction of a second. Other examples are the poor performances on a cancellation test and a test of dexterity (fine eye-hand co-ordination) employed in an earlier study. This differential impairment of perceptual-motor skills is in line with some results obtained at Princeton where subjects were placed in a dark and soundproof room for periods up to three days. In this study depth perception was not impaired while performance on a mirror drawing and a pursuit rotor task was (Vernon, McGill, Gulick, & Candland, 1959). Our results also agree to some extent with those obtained at McGill under conditions of diffuse light and noise. The McGill workers reported no impairment in rate of figure reversal, c.f.f., visual acuity, phi-phenomenon, autokinetic effect, and various brightness phenomena. In contrast to our findings they

reported an impairment of size constancy (Doane, Mahatoo, Heron, & Scott, 1959). Some differences, however, might be expected since the sensory conditions in the two experiments were different.

None of our subjects reported any *gross* perceptual changes involving distortion of size and shape of objects immediately upon emerging from isolation. This is in agreement with the Princeton studies (Vernon & Hoffman, 1956) and also with those carried out at the Aerospace Medical Laboratory, Ohio, where a group of pilots were kept in a dark, soundproof chamber for periods ranging from 4 hours to a week (Ruff & Levy, 1959). These results on darkness and silence are quite different from those reported at McGill where subjects were exposed to diffuse light and noise for periods ranging from 2 to 6 days. Most of these subjects reported gross distortions of the size and shape of objects present in the laboratory and a fluctuation, drifting, and swirling of objects and surfaces in the visual field (Heron, Doane, & Scott, 1956; Doane, Mahatoo, Heron, & Scott, 1959). These results provide a further suggestion that prolonged darkness and silence and prolonged diffuse light and noise may produce different behavioural effects.

Although hallucinations were reported by the majority of our subjects, they were almost exclusively of the simple Type I variety, were in general of short duration, occurred relatively infrequently, and did not appear until after two days in isolation. These hallucinations have been reported by some investigators but not by others. The presence or absence of hallucinatory-like experiences seems to depend on several variables. One of the more important of these is the amount of movement which the subject is permitted. In experiments where no restriction on movements are applied, hallucinatory phenomena seem to be totally or almost totally absent (Vernon, McGill, & Schiffman, 1958; Ruff & Levy, 1959; Smith & Lewty, 1959) while in experiments such as ours where the subject is requested to restrict his movements inside the chamber, hallucinatory activity seems to be quite common (Bexton, Heron, & Scott, 1954; Goldberger & Holt, 1958; Silverman, Cohen, Bressler, & Shmavonian, 1958; Wexler, Mendelson, Leiderman, & Solomon, 1958; Freedman & Greenblatt, 1959). Furthermore, when these restrictions on movement are severe, hallucinations may be reported within a few hours (Lilly, 1956; Wexler, Mendelson, Leiderman, & Solomon, 1958). The second factor which is important in determining the presence or absence of hallucinatory phenomena is visual stimulation, especially diffuse unpatterned light. The role of this variable can be clearly seen in one of the McGill experiments. When some of the good "hallucinators" were placed in darkness their hallucinations either completely disappeared or were greatly diminished. Upon re-exposure to diffuse light the hal-

lucinations returned to their original level of intensity (Doane, Mahatoo, Heron, & Scott, 1959). These results are also in line with those of Ruff and Levy (1959) who kept pilots in constant darkness for periods of up to a week. Few hallucinations were reported. The importance of some visual stimulation is also seen in the Princeton studies in which, during complete blackout conditions, only 1 out of 11 subjects reported hallucinations. However, in an experiment where the blackout condition was not completely successful, due to light leaks in a faulty blindfold, 6 out of 9 subjects reported the presence of simple hallucinations (Vernon, McGill, & Schiffman, 1958). These results are in agreement with our findings where simple hallucinations occurred under conditions involving occasional intrusions from a dim red light.

One of the intriguing findings of this study was the presence, in several cases, of some theta activity particularly in the temporal lobe region. This possible involvement of the temporal lobes is of some interest since it is in line with some observations of Baldwin, Lewis, and Frost (1957). They found that when chimpanzees were subjected to bilateral removal of the temporal lobes prior to 13 days of isolation (darkness and quiet) they exhibited none of the emotional, perceptual, and motor disturbances which non-operated control animals showed after the same type of restriction. Furthermore, these operated animals were immune to the effects of lysergic acid which in controls produced some striking behavioural changes. This experiment suggests that there is some relationship between the possession of normal temporal lobes and the effects of either sensory isolation or lysergic acid in animals. It also suggests that some of the human sensory deprivation effects may have a temporal lobe origin. The impairment of recent memory, which we reported in an earlier paper, may be one of these cases. This suggestion is supported by some research at the Montreal Neurological Institute where patients with temporal lobe lesions, particularly in the hippocampal region, exhibited severe and long lasting impairments of recent memory (Penfield & Milner, 1958). Some of the perceptual changes may also have a temporal lobe origin. This is suggested by reports on the ictal patterns of temporal lobe epilepsy which are often characterized by illusions of size, shape, colour, and depth (Kennedy, 1911).

Earlier in the discussion it was suggested that prolonged darkness and silence and prolonged diffuse light and noise may produce different perceptual effects. This was indicated by the greater perceptual distortions and more frequent and complex hallucinatory-like experiences which appear under diffuse light and noise. These two conditions also seem to differ in their effects on intellectual processes. Our first experiment on darkness and silence (Zubek, Sansom, & Prysiazniuk, 1960) as

well as the experiments at Princeton (Vernon & Hoffman, 1956; Vernon & McGill, 1957) showed little if any impairment of various intellectual abilities, apart from recent memory, whereas experiments on prolonged unpatterned light and noise (Scott, Bexton, Heron, & Doane, 1959) seem to produce widespread intellectual impairment. Although these differences seem to occur after prolonged isolation, there are indications that they are not present after short isolation intervals. This is indicated in an experiment on recognition thresholds for 5-digit numbers where no reliable differences were found between 5 to 30 minute periods of blackout and diffuse light conditions (Rosenbaum, Dobie, & Cohen, 1959). Similarly, Freedman and Greenblatt (1959) in an 8-hour isolation experiment reported no differences between blackout and diffuse light conditions in the production of hallucinatory-like imagery or in the kind and amount of cognitive effects. There were, however, more distortions of simple forms under constant diffuse light. In a further study Freedman and Held (1960) reported no differences between the two conditions in "perceptual lag." The absence of behavioural differences in these experiments is not surprising in view of the shortness of the isolation periods.

The behavioural differences which occur after prolonged isolation may partly be due to differences in central neural activity brought about by the two types of sensory conditions. This view is supported by some exploratory EEG studies in our laboratory which suggest that although the EEG changes occurring under blackout and diffuse light conditions are not too different with isolation periods up to a week, they may be quite different beyond this period. This can best be illustrated by reference to one subject who at the end of 10 days of darkness and silence showed an EEG pattern characterized mainly by fast activity at 12 to 20 c.p.s. together with an abnormal amount of theta activity, particularly in the temporal lobes. However, this same subject when retested a year later showed, after 10 days of diffuse light and white noise, a quite different pattern of EEG activity. It was now characterized by a slowing of the alpha by several cycles together with an excess of theta activity in the temporal lobes. Fast activity of 12 to 20 c.p.s. was nowhere in evidence. In view of these differences in the pattern of neural activity, some behavioural differences might be expected. These results, however, are of an exploratory nature and too much reliance cannot be put on them until more EEG data are available.

In summary we can conclude that exposing subjects to prolonged darkness and silence can result in perceptual changes, appearance of hallucinatory-type experiences, changes in emotionality, impairment of recent memory, and significant changes in EEG activity, all of which point to considerable disorganization of brain function. There is as yet

no adequate explanation of these phenomena or of those reported by other investigators. However, one widely held explanation (Heron, 1957) is based on the view that the normal functioning of the brain depends on a continuing arousal reaction produced in the reticular formation, which in turn depends on constantly varying sensory stimulation. When this variability of sensory input is reduced, as it is in constant darkness and silence or diffuse light and noise, these sensory stimuli lose their power of arousal. Under these conditions the activity of the brain may be impaired and disturbances of psychological processes may occur. There is considerable neurophysiological evidence in favour of this view. However, this explanation has to be modified somewhat in the light of an experiment by Davis, McCourt, and Solomon (1960) who presented *random* visual stimulation to their subjects confined in a tank-type respirator, and observed emotional disturbances, intellectual impairments, and hallucinatory phenomena similar in nature to those occurring under constant light and noise. They conclude that "what the brain needs for normal functioning is not quantity or change in sensation *per se*, but a continuous meaningful contact with the outside world." Freedman and Greenblatt (1959), on the basis of their work on blackout and diffuse light conditions, also suggest that it is the "absence of order and/or meaning rather than the specific nature of the stimulus field which tends to degrade perceptual organization." It would appear, therefore, that what is required for normal functioning of the brain is constantly varying *meaningful* stimulation. When meaning is absent or is reduced for long periods of time, for example under darkness, silence, diffuse light, white noise, or random visual stimulation, psychological disturbances, as we already know, will occur. Furthermore, since these sensory conditions are not equal in meaningfulness they might be expected to produce somewhat different behavioural effects. This has already been shown in some experiments. This explanation, at this stage of our knowledge, can be only an incomplete one. It does not account for many of the sensory deprivation effects. However, when a satisfactory theory eventually emerges, variability and meaningfulness of stimulation will undoubtedly occupy a central role.

Summary

Sixteen subjects were placed in a dark and soundproofed chamber for a week or longer. A battery of perceptual-motor tests was administered before and immediately after they emerged from isolation. In addition Ss were asked to estimate the passage of 1, 3, 5, 15, 30, 60, and 120 minutes at daily intervals during isolation. A carefully matched group of control Ss was given the same tests at the same time intervals. There was no significant impairment of depth perception, size constancy, auditory discrimination, or reversible figures. Furthermore, there were no gross perceptual distortions present when they emerged from isolation. Performance on a visual

vigilance task was impaired significantly. There was no impairment on the time estimation tests except on the 120-minute interval in which the experimentals showed more underestimation than did the controls.

Eleven out of the 16 *Ss* experienced some hallucinatory phenomena at some time during isolation. They were almost exclusively of the simple Type I variety, were in general of short duration, occurred relatively infrequently and appeared only after at least two days of isolation.

REFERENCES

Baldwin, M., Lewis, S. I., & Frost, L. L. Perceptual interference after cerebral ablation. *Perceptual & Motor Skills*, 1957, **7**, 45–8.

Bexton, W. H., Heron, W., & Scott, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.*, 1954, **8**, 70–6.

Bindra, D., & Waksberg, Helene. Methods and terminology in studies of time estimation. *Psychol. Bull.*, 1956, **53**, 155–9.

Clark, B., & Graybiel, A. The break-off phenomenon. *J. Aviat. Med.*, 1957, **28**, 121–6.

Davis, J. M., McCourt, W. F., & Solomon, P. and S. I. The effect of visual stimulation on hallucination and other mental experiences during sensory deprivation. *Amer. J. Psychiat.*, 1960, **116**, 889–92.

Doane, B. K., Mahatoo, W., Heron, W., & Scott, T. H. Changes in perceptual function after isolation. *Canad. J. Psychol.*, 1959, **13**, 210–19.

Freedman, S. J., & Greenblatt, M. Studies in human isolation. *Tech. Rep.*, 59–266, Wright-Patterson Air Force Base, Ohio, WADC Aero-Medical Lab., 1959.

Freedman, S. J., & Held, R. Sensory deprivation and perceptual lag. *Perceptual & Motor Skills*, 1960, **11**, 277–80.

Goldberger, L., & Holt, R. R. Experimental interferences with reality contact (perceptual isolation). I. Method and group results. *J. Nerv. & Ment. Dis.*, 1958, **127**, 99–112.

Heron, W. The pathology of boredom. *Sci. Amer.*, 1957, **196**(1), 52–6.

Heron, W., Doane, B. K., & Scott, T. H. Visual disturbances after prolonged perceptual isolation. *Canad. J. Psychol.*, 1956, **10**, 13–18.

Kennedy, F. The symptomatology of temporosphenoidal tumors. *Arch. Intern. Med.*, 1911, **8**, 317–50.

Lilly, J. C. Mental effects of reduction of ordinary levels of physical stimuli on intact healthy persons. *Psychiat. Res. Rep.*, 1956, **5**, 1–9.

MacLeod, R. M., & Roff, M. F. An experiment in temporal disorientation. *Acta Psychologica*, 1936, **1**, 381–423.

Penfield, W., & Milner, B. Memory deficits produced by bilateral lesions in the hippocampal zone. *A.M.A. Arch. Neurol. & Psychiat.*, 1958, **79**, 475–97.

Rosenbaum, G., Dobie, S. I., & Cohen, B. D. Visual recognitive thresholds following sensory deprivation. *Amer. J. Psychol.*, 1959, **72**, 429–33.

Ruff, G. E., & Levy, E. Z. Psychiatric research in space medicine. *Amer. J. Psychiat.*, 1959, **115**, 793–7.

Scott, T. H., Bexton, W. H., Heron, W., & Doane, B. K. Cognitive effects of perceptual isolation. *Canad. J. Psychol.*, 1959, **13**, 200–9.

Silverman, A. J., Cohen, S. I., Bressler, B., & Shmavonian, B. M. Hallucinations in sensory deprivation. Presented at the "Symposium on Hallucinations"; AAAS meeting, December 26–27, 1958, Washington, D.C.

SMITH, S., & LEWTY, W. Perceptual isolation using a silent room. *Lancet*, 1959, No. 7098, 342–5.

VERNON, J., & HOFFMAN, J. Effect of sensory deprivation on learning rate in human beings. *Science*, 1956, **123**, 1074–5.

VERNON, J., & MCGILL, T. E. The effect of sensory deprivation upon rote learning. *Amer. J. Psychol.*, 1957, **70**, 637–9.

VERNON, J., MCGILL, T. E., & SCHIFFMAN, H. Visual hallucinations during perceptual isolation. *Canad. J. Psychol.*, 1958, **12**, 31–4.

VERNON, J., MCGILL, T. E., GULICK, W. L., & CANDLAND, D. K. Effect of sensory deprivation on some perceptual and motor skills. *Perceptual & Motor Skills*, 1959, **9**, 91–7.

WEXLER, D., MENDELSON, J., LEIDERMAN, P. H., & SOLOMON, P. Sensory deprivation, a technique for studying psychiatric aspects of stress. *Arch. Neurol. & Psychiat.*, 1958, **79**, 225–33.

ZUBEK, J. P., SANSOM, W., & PRYSIAZNIUK, A. Intellectual changes during prolonged perceptual isolation (darkness and silence). *Canad. J. Psychol.*, 1960, **14**, 233–42.

(*Received December 28, 1960*)

THE EFFECT OF PRACTICE ON UTILIZATION OF INFORMATION FROM POSITIVE AND NEGATIVE INSTANCES IN CONCEPT IDENTIFICATION[1]

VAIRA FREIBERGS AND ENDEL TULVING
University of Toronto

THE CONCEPT IDENTIFICATION EXPERIMENT can be regarded as a communication situation in which the experimenter gives information to a subject about the concept to be identified through a series of messages (Hovland, 1952). A message typically consists of a stimulus figure involving a combination of characteristics. If all the defining characteristics of the concept are included in the stimulus figure, the message represents a positive instance of that concept. If one or more such characteristics are lacking, the message represents a negative instance of the concept.

Hovland and Weiss (1953) have conducted a well-known series of experiments on transmission of information through positive and negative instances. They used Hovland's (1952) communication model of concept attainment to calculate the minimum number of positive or negative instances logically required to communicate the concept to the subject. They then compared the performance of subjects working with only positive instances with that of subjects working exclusively with negative instances, always holding constant the *amount* of information presented to the subjects. They found that, within the time limits they used, a significantly greater percentage of subjects working with positive instances than of subjects working with negative instances were able to identify the concepts correctly. This was true even when the total number of instances presented was equal for both groups and when the memory factor was eliminated. This led the authors to conclude that "while a machine could be constructed which would arrive at the correct concept with equal ease on the basis of the positive or negative instances, the results of the present experiment clearly indicate that the human organism does not operate similarly on a strict probability basis" (Hovland and Weiss, 1953, p. 181).

This is a rather broad generalization which has found support by other investigators. Bruner, Goodnow, and Austin, for instance, also state that "subjects seem not as willing or able to use negative information—instances telling what the concept is not—in the process of attaining a concept" (1956, p. 180).

[1]This study is based on a thesis submitted by Vaira Freibergs (née Vikis) to the University of Toronto for the degree of M.A.

Hovland and Weiss seem to favour a perceptual explanation of this phenomenon, or at least an explanation that includes references to perceptual qualities of positive and negative instances. It is conceivable, however, that there are other major sources of variability which are relevant. Hovland and Weiss' experimental findings can be regarded, for instance, as reflecting transfer effects from prior, unspecified learning of concepts. Bruner, Goodnow, and Austin have suggested that "most of our environment seems to be geared to working with positive instances and with variations on these" (1956, p. 159). If this hypothesis is true, then one would not only expect that experimentally naïve subjects show differences in handling positive and negative instances, but also that these differences disappear when the subjects are given more extensive practice with negative instances. The present experiment, essentially a replication of a part of Hovland and Weiss' Experiment III, was conducted to explore the effect of repeated presentation of problems on utilization of positive and negative instances.

Method

Subjects

Twenty Ss were haphazardly drawn from the subject pool consisting of the names of all students taking general psychology courses at the University of Toronto. They were 18 women and two men, all between 18 and 26 years of age. None of them had any familiarity with concept identification tasks of any kind prior to the experiment. The Ss were alternately assigned to two groups in the order of their appearance for the experiment.

Task and procedure

The stimulus material consisted of 64 cards similar to those used in the Wisconsin Card Sorting Test (Grant, 1951). On each card there were one, two, three, or four solid geometric figures of a certain form and a certain colour. Forms used were circle, triangle, square, and diamond, and colours were blue, green, yellow, and red. Thus there were three stimulus dimensions—number, form, and colour, with four values within each dimension.

All concepts to be identified by Ss in this experiment involved three values within each of two relevant dimensions. Thus in each case one dimension was irrelevant and one value within each relevant dimension was "incorrect."[2] The Ss' task was to identify a given concept on the basis of four instances presented simultaneously. One group of Ss (P Group) worked with only positive instances, the other (N Group) had to arrive at the concepts on the basis of negative instances alone. As shown by Hovland (1952), four positive or four negative instances of the type of concept used in this experiment are logically sufficient to communicate the concept to S.

Both groups were given 20 successive problems ("trials") involving different concepts of the same general type, that is, defined by three values within each of two relevant dimensions. The particular concept assigned to a given S on each trial

[2]For a detailed discussion of such concepts, see Hovland (1952).

was drawn at random from a list of 48 concepts of the designated type within the limits of the stimulus material. For each such concept there are 36 cards in the deck of 64 that represent positive instances and 28 cards that represent negative instances of that concept. The four instances of a given concept presented to S on each trial were selected in a haphazard fashion designed to minimize any possible systematic effects attributable to particular combinations of appropriate instances, and subject to the requirement that the four instances unequivocally provide sufficient information to identify the concept.

Instructions

Extensive and detailed instructions, identical for all Ss, were given verbally by *E*. The terms "concept," "dimension," "value," "positive instance," and "negative instance" were defined and illustrated with the aid of stimulus cards.[3] The Ss were shown how one could arrive at a concept defined by three values within each of two relevant dimensions on the basis of both four positive and four negative instances. Appropriate parts of instructions were repeated until S indicated that he understood the general task. Only then was S informed whether he would be working with positive or negative instances, and that for each set of four positive instances (negative for the N Group) presented to him he should attempt to give the correct concept as fast as he could. To minimize the need to rely on memory, a chart with a diagrammatical representation of the three dimensions and the four values within each dimension was given to each S and he was permitted to refer to it throughout the experiment.

Dependent variable

The Ss' performance was measured by the time taken to specify a concept correctly on a given trial. The scores were expressed in sec. to the nearest sec. A stop-watch was started the moment the four cards representing either positive or negative instances were laid on the table in front of S. The stop-watch was stopped when S stated the three correct values within each of the two relevant dimensions, or when the time limit of 210 sec. per trial was reached. This time limit was selected on the basis of preliminary observations. The stop-watch was not stopped when a wrong answer was given within the allowed time limit. In this case S was simply informed that his answer was not correct. When S had not identified the concept correctly at the end of 210 sec. he was assigned the score "210 plus" on that trial. The *E* then presented the instances of the next problem and said, "All right, now let us try this one," but at no time did he tell S what the correct answer was.

Results

All subjects in the P Group identified all concepts correctly after the third trial, and all subjects in the N Group did so after the eleventh trial. During the early trials, however, a number of subjects in both groups could not give the correct answer within the 210 sec. time limit. For this reason, and also because the distributions of individual scores on most trials were skewed, median scores were used to compare the performance of the two groups. These data are shown graphically in Figure 1.

[3]A copy of the instructions is available from E. Tulving, Department of Psychology, University of Toronto.

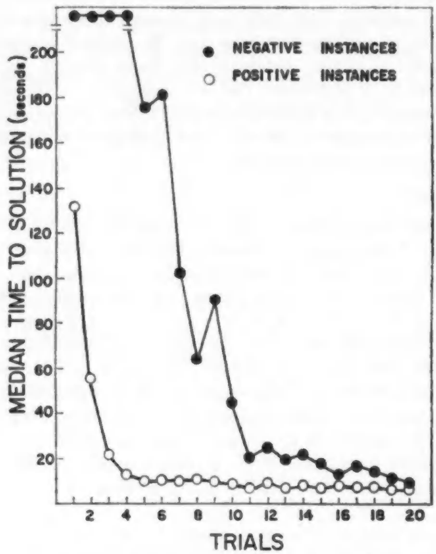

FIGURE 1. Median times to solution of concept identification problems on 20 successive "trials" for the groups working with positive and negative instances.

The abscissa of Figure 1 represents 20 successive problems or trials, the ordinate shows the median times to solution. The upper curve is that of the N Group, the lower shows the performance of the P Group. The median for the N Group on the first four trials is larger than 210 sec. and thus indeterminate. Hence the first four points on the curve for the N Group do not correctly represent the data and are shown only for the sake of completeness of the graph.

Both groups can be seen to improve in performance considerably during the total practice period. The two learning curves are negatively accelerated, but the curve of the P Group is much steeper than that of the N Group. The differences between the two groups during the early trials are large and obvious. At the end of the practice period, however, the difference between the two medians is quite small, especially when compared with the initial difference. On trial 20, for instance, nine subjects in the P Group and seven subjects in the N Group identified the concept in ten seconds or less. The median of the P Group on the same trial was 6.5 sec., the median of the N Group was 9.5 sec. It also looks

as if the P Group had almost reached the level of asymptotic performance, whereas the N Group still seemed to be improving by the 20th trial.

No statistical tests were performed on these data. First, we know no appropriate statistical tests that could be used with these data to test for the interaction effects between practice and type of instances, which seems to be strongly suggested by the data. Secondly, in view of the regularity of the median curves, the conclusion that the difference between positive and negative instances depends on the amount of practice the subjects have had, seems quite reasonable even without any tests.

Discussion

There are two main findings in this study and we shall briefly examine them in turn. First, a subject's ability to solve concept identification problems (ability to think?) is very greatly affected by practice. This is true for both positive and negative instances. With median time to solution as the dependent variable the performance of both relatively small samples used in this study improved approximately by a factor of 20, over something like an hour's practice. In view of such huge practice effects it is very surprising that workers in the field of concept formation have neglected this aspect of the general problem. We are aware of only one paper in which improvement in ability to solve problems of the same general type has been shown to improve with practice (Archer, Bourne, & Brown, 1955), but even that finding was incidental to the authors' main interest in the effect of irrelevant information and instructions.

This experiment sheds no light on the problem of how these practice effects are mediated. It may be that continued exposure to the stimulus cards and repeated attempts at solution bring about a certain kind of perceptual reorganization or establish a different perceptual set, but at the present stage of our ignorance this is entirely a matter of conjecture. Psychologists in general know little about the mechanism of perceptual learning (Drever, 1960), and the problem of practice effects in cognitive processes is usually not even mentioned (Leeper, 1951), let alone understood.

The second main finding of this experiment pertains to the effectiveness of positive and negative instances. Although during the early stages of practice the subjects seemed much less capable of assimilating information from negative than from positive instances, the difference was very much smaller at the end of 20 trials. Thus these data both confirm and to a certain extent circumscribe Hovland and Weiss' conclusions. Positive instances in a concept identification task are used more efficiently than negative ones by subjects who are relatively unsophisticated with respect

to concept identification tasks, but for more practiced subjects the *nature* of the information concerning concepts seems to be less relevant and presumably only the *amount* of information is important. This finding is entirely consistent with the view that the initial difference between positive and negative instances reflects transfer effects from previous learning of concepts outside the laboratory.

It is conceivable, considering the trend of data in Figure 1, that with additional practice the remaining small difference would disappear. It is equally possible, of course, that even at the asymptote positive instances would retain a small but significant advantage over negative ones. This clearly is a problem for future experiments. Until the role of practice on utilization of positive and negative instances has been more carefully investigated, however, it would seem somewhat premature to be overly concerned with the effect of other variables on the apparent difference.

Summary

A series of concept identification problems was given to two groups of Ss in order to determine the effect of practice on utilization of positive and negative instances in attaining concepts. One group worked with only positive instances, the other with only negative instances. Considerable improvement in performance was found for both groups during the practice period. While the performance of the positive group was markedly superior at the outset, by the end of 20 trials the two groups were quite similar in their median performance. The findings suggest that the effectiveness with which Ss can handle both positive and negative instances in concept attainment depends on the amount of previous practice on similar tasks.

REFERENCES

Archer, E. J., Bourne, L. E., Jr., & Brown, F. G. Concept identification as a function of irrelevant information and instructions. *J. exp. Psychol.*, 1955, 49, 153–64.

Bruner, J. S., Goodnow, J. J., & Austin, G. A. *A study of thinking*. New York: Wiley, 1956.

Drever, J., 2nd. Perceptual learning. *Ann. Rev. Psychol.*, 1960, 11, 131–60.

Grant, D. A. Perceptual versus analytical responses to the number concept of a Weigl-type card sorting test. *J. exp. Psychol.*, 1951, 41, 23–9.

Hovland, C. I. A "communication analysis" of concept learning. *Psychol. Rev.*, 1952, 59, 461–72.

Hovland, C. I., & Weiss, W. Transmission of information concerning concepts through positive and negative instances. *J. exp. Psychol.*, 1953, 45, 175–82.

Leeper, R. Cognitive processes. In S. S. Stevens, ed. *Handbook of experimental psychology*. New York: Wiley, 1951.

(*Received October 3, 1960*)

NEGATIVE REINFORCEMENT OF TWO GRAMMATICAL RESPONSE CLASSES

HENRI C. BARIK[1] AND SAMUEL FILLENBAUM
University of North Carolina

IN RECENT YEARS there have been a number of studies on the experimental manipulation of verbal behaviour (see reviews by Adams, 1957; Krasner, 1958; Salzinger, 1959). The majority of these studies have used positive reinforcement to effect changes in the rate of emission of the "conditioned" response. Little has been reported on the use of negative reinforcement, or on the conditions in which negative reinforcement is effective.

Eriksen and Kuethe (1956) obtained avoidance conditioning in word association by giving shock after certain responses. Investigating verbal reinforcers Greenspoon (1955) found that the rate of emission of plural nouns could be decreased by saying "huh-uh" after such responses. This negative reinforcer, however, was ineffective when used for non-plural words. Verplanck (1955) showed that the frequency of opinion-statements in a subject's utterances could be decreased by expressing disagreement whenever such a statement was made.

The present study was an attempt to condition through negative reinforcement the response units described by Barik and Lambert (1960). In their study, subjects were required to complete 80 sentences of a standard grammatical form, "This is the table that–" serving as an example. The completions were classified as either Subject or Non-subject responses, depending upon whether or not the relative pronoun "that" was made the subject of the subordinate clause (e.g., Subject response: This is the table that has a broken leg; Non-subject response: This is the table that John built). Barik and Lambert found that by saying "good" whenever the subject gave a Non-subject response it was possible to increase the rate of emission of this response class (the performance of the experimental group being compared with that of a control group which received no reinforcement). The procedure, however, was ineffective for Subject responses.

In this study, instead of using an established negative reinforcer such as shock, a procedure was followed which involved the continued repetition of an item until a response of a certain kind, previously defined as correct, occurred. Thus, reasoning that repeated presentation of the same task or item might lead to satiation on, and avoidance of, the item, we investigated the possible aversive properties of continued stimulus repetition.

[1]Now with the Allan Memorial Institute, Montreal.

CANAD. J. PSYCHOL., 1961, 15(2)

Method

Materials

The 80 sentences used in this study were the same as those listed in the Barik and Lambert study (1960), each sentence being typed in block letters on a 3 in. × 5 in. white index card.

Subjects

Ss were 60 students from the introductory psychology course at the University of North Carolina who were assigned at random to one of three groups of 20 each: a Subject group, a Non-subject group, and a control group which served as control for both situations. There were ten male and ten female Ss in each group. All Ss were tested by the same *E*.

Procedure

The experiment was carried out in a small testing room containing a table and two chairs on opposite sides of the table. A heavy cardboard panel erected in the middle of the table prevented S from seeing *E*. A small horizontal aperture, permitting *E* to slide a card to S, was cut out in the centre of the base of the panel. S was given a sheet of instructions and told to follow them carefully as *E* read them aloud to him. The instructions were as follows:

"The purpose of this experiment is to investigate the nature of verbal reactions to visually presented material.

I shall be seated on the other side of this panel, and I shall slide a number of cards to you through this aperture, one at a time. You are to take the card, look at the unfinished sentence that is typed on it, and complete that sentence by adding a few words to it. Please say the first thing that comes to mind. When you have completed the sentence, return the card to me through the aperture and I shall give you another one.

I shall give you two examples to show you what you have to do.

Pick up the card which I have just passed you. On it you will see the sentence 'This is the book that—.' All you have to do is repeat it and complete it by adding a few words. For example, you could say, "This is the book that I had to buy for one of my courses,' if that is what you thought of. O.K.? Now, please return the card through the aperture.

Here's another card. On it you will see the sentence, 'This is the sign that—,' and what you do is read it and add a few words, as for example, 'This is the sign that hangs on the wall,' if that's what you think of. O.K.? Please return the card to me.

Sometimes the same sentence may be shown to you time after time. You are to respond to each new presentation of the same sentence, and on each occasion complete the sentence by whatever you think of at the time of that particular presentation. Is that clear? [The preceding paragraph was not read to Control Ss, but only to the Ss in the two experimental groups.]

Do you have any questions? Be sure you understand what you have to do, because I cannot answer any question during the experiment. Also, please speak loud and clearly when you are completing the sentences."

The order of presentation of the 80 sentences, divided into four blocks of 20 trials each, was randomized from S to S by using four different orders of the four blocks of 20 trials, the order of the sentences within each block remaining the same. An equal number of Ss in each of the three groups received the same order of sentence presentations.

A short interval of approximately 5 sec. elapsed between S's response to one sentence and the presentation of the next (whether the next sentence was the same or different).

The Non-subject group was negatively reinforced for Non-subject responses. In this group, whenever S completed a sentence with a Non-subject response, he was given the same sentence over and over again until he made the other sort of completion, that is, gave a Subject response. The purpose of this was to see if such a procedure would lower the frequency with which sentences were completed by a Non-subject response. Once the sentence was Subject-completed a new sentence was presented. For the Subject group a comparable procedure was followed, the same sentence being presented over and over as long as it was completed by a Subject response, that is, until it had been completed by a Non-subject response. There was, however, a limit set to the number of continuous presentations of a sentence. If the "correct" response (Subject response in the Non-subject group, Non-subject response in the Subject group) had not been made by the fifteenth continuous presentation of a sentence, *E* moved on to the next sentence in the list. For both Subject and Non-subject groups the first twenty trials, which served to establish the operant level of the response, were presented only once. Repeated presentation of a sentence completed incorrectly was begun on the twenty-first trial. The control group was given each sentence only once, no matter what the completion.

E recorded verbatim enough of each completion to permit determination of the response class to which it belonged.[2] Frequency of incorrect response (Subject response in the Subject group, Non-subject response in the Non-subject group) on first presentation of each stimulus sentence constituted the main dependent variable.

At the end of the experimental session, Ss in the experimental groups were asked to complete a questionnaire, the purpose of which was to find out whether S had any insight into the experimental situation and to determine whether or not S found continued repetition of a sentence aversive. Among the questions asked were the following: How did it affect you when some given sentence was presented over and over? Did you not mind it, did you enjoy having the same sentence repeated several times on end, or were you annoyed by it and wish to go on to a new sentence?

Results

Of the 40 experimental subjects only two (one in each experimental group) showed any insight into the relation between their response to a sentence and the nature of the next sentence presented. Data from these subjects were not considered further. No subject revealed any awareness of the real purpose of the experiment. The majority of subjects expressed annoyance at having the same sentence presented time after time. Only six "did not mind it" or "enjoyed it," but since their data did not show any departure from those of the other subjects they were not treated separately in the analysis.

An analysis of variance on the operant level of each form of response

[2]Some arbitrary decisions (specified prior to experimentation) had to be made for poorly constructed completions; fortunately there were few such sentences.

(Subject and Non-subject) in the three groups (with each group subdivided on the basis of sex) failed to show any differences among groups ($F = 1.13$ with 5 and 52 df). Nor did the two experimental groups differ in the number of incorrect responses on operant level trials ($F = 1.44$ with 3 and 34 df).

For every block of 20 trials the mean number of incorrect responses in each experimental group, and of each form of response in the control group, is shown in Table I. For the experimental groups, the figures reported for trials 21–80 represent the mean number of sentences or trials in each block completed incorrectly on the first presentation of the sentence, or the mean number of sentences having to be presented more than once. The last column in Table I gives the average number of incorrect responses per block of 20 trials over trials 21–80, the reinforced trials for the experimental groups.

TABLE I

MEAN FREQUENCY OF OCCURRENCE OF RESPONSES ON FIRST SENTENCE PRESENTATION FOR EACH BLOCK OF 20 TRIALS

		Trials				
		1–20	21–40	41–60	61–80	$\overline{\text{R.T.}}$*
Non-subject group						
Non-subject responses	$\bar{X}$	10.32	8.21	9.53	7.95	8.57
	S.D.	4.04	3.74	4.43	3.82	3.48
Subject group						
Subject responses	$\bar{X}$	8.84	8.05	9.74	9.79	9.19
	S.D.	3.11	3.73	4.17	3.88	3.32
Control group						
Non-subject responses	$\bar{X}$	9.35	8.00	8.40	8.80	8.40
Subject responses	$\bar{X}$	10.65	12.00	11.60	11.20	11.60
	S.D.	3.63	3.63	3.57	4.10	3.15

*Mean frequency of occurrence per block of 20 trials for trials 21–80.

To determine the significance of the changes observed in the experimental groups in the mean number of incorrect responses, the change in the frequency of occurrence of the incorrect response from trials 1–20 (operant level trials) to each subsequent block of 20 trials was tabulated for each subject in each of the two groups, as well as for the subjects in the control group, and the changes in the experimental groups were compared with those in the control group (for their respective response) by means of uncorrelated t-tests. The results of this analysis are shown in Table II. Only the change (decrease) in the Subject group in the fre-

quency of occurrence of Subject responses from Block 1 to Block 2 is significantly greater than that shown by the control group for the same trials. Neither of the two experimental groups shows a significant over-all decrease in incorrect responses from operant level trials to reinforced trials, when their performance is compared with that of the control group (last column of Table II).

TABLE II

DIFFERENCES BETWEEN EXPERIMENTAL AND CONTROL GROUPS IN CHANGE IN MEAN NUMBER OF INCORRECT RESPONSES FROM FIRST BLOCK OF TRIALS (OPERANT LEVEL) TO SUBSEQUENT BLOCKS—t VALUES.

			Blocks of trials			
	d.f.		Bl. 2–1	Bl. 3–1	Bl. 4–1	Bl. $\overline{\text{R.T.}}$ – 1
Non-subject group vs. control group on Non-subject responses	37	t =	−0.62	0.14	−1.56	−0.83
Subject group vs. control group on Subject responses	37	t =	−2.06*	−0.05	0.31	−0.62

*p = .05

In addition to considering the correctness or incorrectness of the first response to each stimulus sentence one may inquire whether, as the experimental session progresses, there is any change (decline) in the average number of incorrect responses to each sentence before the correct response finally occurs. A further analysis was carried out on the *total* number of responses (both correct and incorrect) made by the experimental groups in each block of reinforced trials. The median number of responses per different stimulus sentence for each reinforced block was calculated for each subject. Conditioning would be shown if the median number of responses per different stimulus sentence decreased from the first reinforced block (trials 21–40) to succeeding blocks, that is, if fewer incorrect responses were emitted before the correct response was made as the experiment progressed. No significant differences were found among the three median distributions when either the sign test or Wilcoxon's signed ranks test was used.

Table III shows the t-values obtained when comparing differences between the Subject and Non-subject groups in change in the mean number of incorrect responses from operant level trials to each block of reinforced trials. The t of —2.39 reported in the last column is significant at the 5 per cent level, indicating that the change (decrease) in mean

TABLE III

DIFFERENCES BETWEEN NON-SUBJECT AND SUBJECT GROUPS IN CHANGE IN MEAN NUMBER OF INCORRECT RESPONSES FROM FIRST BLOCK OF TRIALS (OPERANT LEVEL) TO SUBSEQUENT BLOCKS—t VALUES.

			Blocks of trials			
	d.f.		Bl. 2–1	Bl. 3–1	Bl. 4–1	Bl. $\overline{\text{R.T.}}$ – 1
Non-subject group vs. Subject group	36	$t =$	−1.17	−1.45	−3.19**	−2.39*

*p = .05 **p = .01

number of incorrect (Non-subject) responses per block of 20 trials from operant to reinforced trials in the Non-subject group is significantly greater than the change in mean number of incorrect (Subject) responses for the same trials in the Subject group.[3]

DISCUSSION

The method used here to establish conditioning of the two response forms proved ineffective. Even though something like 85 per cent of the experimental subjects reported a dislike for the procedure of repeated item presentation, this procedure did not have any consistent effect upon response rate since the results of the two experimental groups do not depart significantly from those of the control group. Apparently, the fact that subjects report some operation as aversive does not necessarily imply that such an operation can be used to control behaviour. Whether the process of repeated presentation of a sentence can be made effective as a negative reinforcer by instructing the subject as to its negative attributes might be investigated. This could be done by telling the subject at the beginning of the session that it is "bad" to have some sentence presented more than once, or by using a scoring system whereby points are deducted from the subject's score whenever a sentence is repeated.

The results reported in Table III, which show that the change in the mean number of incorrect responses from operant level trials to reinforced trials is significantly greater in the Non-subject group than in the Subject group ($t = -2.39$), are to be expected in view of the fact that the mean frequency of Non-subject responses per block decreases,

[3]Trend analyses of the data (using the Alexander procedure) yield essentially the same results as those reported in Tables I–III. The F for between group slopes does not reach significance when Subject and control groups are compared on Subject responses, and Non-subject and control groups on Non-subject responses. When the two experimental groups are compared on incorrect responses, however, the F for between group slopes is significant beyond the 1 per cent level.

whereas that of Subject responses increases from the first 20 trials to the remaining trials, not only in the experimental groups but also in the control group (Table I). Since neither of the two experimental groups shows "conditioning" when changes in the frequency of incorrect responses are compared with those of the control group, discussion of differences in the "conditionability" of the two groups is unwarranted. The results of Table III are consistent with those of Barik and Lambert (1960) who found that the change in mean number of Non-subject responses from operant to reinforced trials in the Non-subject group was greater than the change in mean number of Subject responses in the Subject group for those trials, although in their study, since conditioning was achieved through positive reinforcement, increases in response frequency were observed.[4] Thus it appears that the Non-subject form of response may be more affected by the reinforcement procedure, whether the reinforcement is of a positive or negative kind.

The results of this study are of interest in that they raise certain questions as to the control procedure to be used in studies on verbal conditioning. Table I reveals that the Non-subject group showed a decrease in the frequency of Non-subject responses from operant trials to reinforced trials. Yet, this change is not significant when compared with that of the control group for the same trials. If, however, each group is considered separately, and the significance of the changes in each group from operant to reinforced trials is determined by means of correlated *t*-tests a rather different picture emerges (see Table IV). The decrease

TABLE IV

CHANGES IN THE THREE GROUPS IN MEAN NUMBER OF INCORRECT RESPONSES FROM FIRST BLOCK OF TRIALS (OPERANT LEVEL) TO SUBSEQUENT BLOCKS—*t* VALUES.

	d.f.		Blocks of trials			
			Bl. 2–1	Bl. 3–1	Bl. 4–1	Bl. $\overline{\text{R.T.}}$ – 1
Non-subject group						
Non-subject responses	18	$t =$	−2.32*	−1.05	−3.70***	−2.78**
Subject group						
Subject responses	18	$t =$	−1.22	1.01	1.16	0.56
Control group						
Non-subject responses / Subject responses	19	$t =$	∓1.59	∓1.19	∓0.57	∓1.27

*p = .05 **p = .02 ***p = .01

[4]With regard to the comparison of the two experimental groups on reinforced responses in the Barik and Lambert study, there are two minor errors in their Table VI: the number of df's should be 35 instead of 34; the t of 2.02 is not significant, as reported, but just fails to reach significance, $t_{35} = 2.03$ being required for $p = .05$.

in the Non-subject group in the mean number of Non-subject responses per block of 20 trials from operant level trials to reinforced trials is significant beyond the 2 per cent level ($t = -2.78$). The changes in the Subject group are not significant, nor are those in the control group. The results reported in Tables II and IV with regard to Non-subject responses, one set of results showing significant change and the other not, demonstrate that different results may be obtained if the baseline against which response changes are assessed varies. A recent study by Rogers (1960) indicates that significant changes may be observed in the absence of any reinforcement contingency. Reinforcing self-reference verbalizations (positive self-references in one group, negative self-references in another) in a quasi-therapy situation, he found that a control group which received no reinforcement throughout the experiment nevertheless showed a significant decrease in positive self-references from the "operant level" interview to "reinforced" interviews. The experimental group reinforced for positive self-reference did not show a significant increase in such responses from operant to reinforced interviews: however when compared with the control group their change in positive self-references was significant. Thus an experimental group may appear to show conditioning (significant changes) when a comparison is made of its performance in pre-treatment and treatment trials, yet fail to show conditioning when its performance is compared with that of a control group (present study), or it may appear to fail to show conditioning when its performance in operant trials is compared with that in reinforced trials, yet show conditioning when its changes from operant to reinforced trials are compared with those of a control group (Rogers' study). It is obvious that, regardless of reinforcement contingencies, with continued emission of a response over time there may come about changes in response rate (and the conditions making for such response inhibition, extinction, satiation, or increase might be of considerable interest). It seems essential, therefore, that a control group be used in studies of verbal conditioning, and that the change in the performance of the experimental group from pre-treatment to treatment trials should be compared with that of the control group for the same trials. A number of studies (see, for example, Buss, Gerjuoy, & Zusman, 1958; Cohen & Cohen, 1960; Sapolsky, 1960; Wilson & Verplanck, 1956) have failed to use a control group, the analysis being based only on a comparison of the performance of subjects on operant and reinforced trials. In view of the comments made above a re-evaluation of their findings may be warranted.

Summary

Sixty Ss were required to complete 80 sentences of a standard grammatical construction, "This is the table that–." serving as an example. Completions were

dichotomized as Subject or Non-subject responses depending upon whether or not the relative pronoun "that" was made the subject of the subordinate clause. The first 20 trials served to establish the operant level of each type of response. For trials 21–80 one group of 20 Ss (Non-subject group) was given over and over any sentence completed by a Non-subject response, the procedure being continued until a Subject response was made to the sentence. A comparable procedure was used with a second group of 20 Ss (Subject group), any sentence completed by a Subject response being repeated until it was completed by a Non-scbject response. A third control group was given each sentence only once regardless of type of completion.

When the performance of the two experimental groups was compared with that of the control group the procedure of presenting a sentence over and over until the correct response was made proved ineffective as a negative reinforcer, even though most Ss disliked this procedure. In the Non-subject group there was a significant decrease in frequency of incorrect responses from their own operant level. This finding led us to suggest that in any study of verbal conditioning operant level should not be used as a baseline but, rather, that comparisons must be made between response changes under experimental manipulation and changes under control conditions.

REFERENCES

ADAMS, J. K. Laboratory studies of behavior without awareness. *Psychol. Bull.*, 1957, 54, 383–405.

ALEXANDER, H. W. A general test for trend. *Psychol. Bull.*, 1946, 43, 533–57.

BARIK, H. C., & LAMBERT, W. E. Conditioning of complex verbal sequences. *Canad. J. Psychol.*, 1960, 14, 87–95.

BUSS, A. H., GERJUOY, IRMA R., & ZUSMAN, J. Verbal conditioning and extinction with verbal and nonverbal reinforcers. *J. exp. Psychol.*, 1958, 56, 139–45.

COHEN, E., & COHEN, B. D. Verbal reinforcement in schizophrenia. *J. abnorm. soc. Psychol.*, 1960, 60, 443–6.

ERIKSEN, C. W. & KUETHE, J. L. Avoidance conditioning of verbal behavior without awareness: a paradigm of repression. *J. abnorm. soc. Psychol.*, 1956, 53, 203–9.

GREENSPOON, J. The reinforcing effect of two spoken sounds on the frequency of two responses. *Amer. J. Psychol.*, 1955, 68, 409–16.

KRASNER, L. Studies of the conditioning of verbal behavior. *Psychol. Bull.*, 1958, 55, 148–70.

ROGERS, J. M. Operant conditioning in a quasi-therapy setting. *J. abnorm. soc. Psychol.*, 1960, 60, 247–52.

SALZINGER, K. Experimental manipulation of verbal behavior: a review. *J. Gen. Psychol.*, 1959, 61, 65–94.

SAPOLSKY, A. Effect of interpersonal relationships upon verbal conditioning. *J. abnorm. soc. Psychol.*, 1960, 60, 241–6.

VERPLANCK, W. S. The control of the content of conversation: reinforcement of statements of opinion. *J. abnorm. soc. Psychol.*, 1955, 51, 668–76.

WILSON, W. C., & VERPLANCK, W. S. Some observations on the reinforcement of verbal operants. *Amer. J. Psychol.*, 1956, 69, 448–51.

(Received October 25, 1960)

BOOK REVIEWS

The Language of Psychology. By GEORGE MANDLER AND WILLIAM KESSEN. New York: John Wiley & Sons, Inc., 1959. Pp. xviii, 301.

EVEN AS IT IS REPUTEDLY IMPOSSIBLE not to be thrilled by Edgar Wallace, so it is impossible not to be attracted to a book on the language of psychology which begins by quoting James Thurber's dictum "A word to the wise is not sufficient if it doesn't make any sense." The book is addressed to the wise—that is to those who have begun to know how little they know—and its purpose is to present "a systematic treatment of scientific language, definition and theory construction in psychology." It is at once difficult, deft, and distinctive.

It is difficult because the authors, with uncompromising honesty, plunge head on into the tangled, thorny questions which lurk beneath the surface of even the most elegantly designed experimentation, refusing the comfort and security of operational definitions and statistical respectability to raise such awkward questions as the nature of reality (and unreality), how the sciences are to be differentiated, what if anything they have in common, and (worst of all) what is psychology? (The latter it turns out still successfully defies definition, but it *does* have a subject matter, despite dark metaphysical speculation to the contrary.) The question of how and on what basis one gets started on the scientific psychological adventure is raised, together with that of how, since psychology needs a language of its own, it communicates with those disciplines not within its linguistic ambit.

The authors steer a middle course between the Scylla of the monolithic superscience implied by the reductionist position and the Charybdis of private phenomenology. They tilt effectively with that persistent windmill of all the recent and therefore self-conscious sciences, the problem of priorities. No doubt their experience as clinicians has enabled them to avoid the either-or dilemma of painting psychology as fundamental to all other science—a demonstrably absurd but otherwise reassuring position designed to bolster the ego of aggressive psychologists—or of reducing it all to physics, an equally untenable position favoured by the less aggressive who prefer genuflection to cerebration.

These examples will serve to demonstrate how successfully the classic dichotomies of the two-valued, either-or assumptions of the vernacular are avoided, and how as a consequence the book has something to say to the philosophically minded psychologist addicted to such questions

as "Is there a theory of theory making," and the rugged empiricist with his irritated demand to be allowed to get back to his bar presser.

There is, the authors firmly believe, a middle ground on which logical analysis and empirical data combine to produce a psychological language which will be "useful to researchers in the behavioural sciences." This notion, that logic and empiricism combine to produce a scientific language is the essential new insight of the book, the reason why it is not another "logic of psychology." A language has not only a vocabulary but also a set of rules for combining its words to make statements which can in turn be organized into theories. Part one accordingly deals with the problems of vocabulary, definition, and operations; part two with theory, induction, deduction, and explanation.

I am not at all sure how Mandler and Kessen managed to pull this off. After all what language do you use when you wish to write about a second language, when you begin by asserting that the vernacular is inadequate? This is always the problem of the language of criticism—it implies a set of criteria independent of the object criticized but sufficiently specialized and informed to be able to say trenchant and sensible things about that object. In this sense then the language of criticism *is* a third language. It is not psychology and it is not the vernacular. It is also not scientific since it treats of science as an object. Whatever one's view on this threatened regressus, the book meets the pragmatic criterion: it does describe the language of psychology with clarity, vigour, and good humour.

Few psychologists will be unaware of the problems discussed in the book. But fewer, if any, will have achieved anything like the systematization of those problems which it achieves.

One minor irritation. The incessant use of the word "archaic (pp. 9–14–15–18–28–29–47–65–73 *ad nauseam*) belongs to the New Yorker's "Infatuation with sound of own words" department.

D. C. Williams

University of Toronto

Individual Choice Behavior: A Theoretical Analysis. By R. Duncan Luce. New York: John Wiley & Sons, Inc. 1959. Pp. xii, 153. $5.95.

In *Individual Choice Behavior* Dr. Luce presents a significant contribution to the integration of the theoretical concepts underlying such apparently diverse fields of psychological phenomena as psychophysics, motivation, games and gambling, and learning. He starts his analysis with the concept of individual choices, which he defines in terms of the

probabilities of choosing each of a given set of known alternatives. This definition neatly avoids a number of difficult problems, but at least Luce is aware that such problems exist; for example: ". . . all of our choice theories—including this one—begin with the assumption that we have a mathematically well-defined set, the elements of which can be identified with the choice alternatives. How these sets come to be defined for organisms, how they may or may not change with experience . . . are questions that have received but little illumination so far. There are limited experimental results on these topics, but nothing like a coherent theory. Indeed, the whole problem still seems to be floundering at a conceptual level, with us hardly able to talk about it much less to know what experiments to perform" (pp. 3–4). Setting these problems aside and accepting the assumptions and definition, what sort of theory of choice emerges? A set of probability axioms is given (the "ordinary probability axioms") together with an additional axiom, axiom 1, which states a particular relationship amongst the probabilities of choices. The relationship chosen is such as to conform to intuitive notions about how choices amongst two or more alternatives may be related, and on the basis of this axiom the theory of choice is built up. Since the axiom is so central to the whole development it should perhaps be stated in full:

Axiom 1: Let T be a finite subset of U such that, for every $S \subset T$, P_S is defined.

(i) If $P(x,y) \neq 0, 1$ for all $x, y \in T$, then for $R \subset S \subset T$
$$P_T(R) = P_S(R)P_T(S);$$
(ii) If $P(x,y) = 0$ for some $x, y \in T$, then for every $S \subset T$
$$P_T(S) = P_{T-(x)}(S-(x)).$$

There is another good reason for quoting the axiom, which is this: the book is written by a mathematician, who draws on the symbolism of mathematics (matrix algebra, calculus notation, etc.) where necessary, which means that it will be difficult to read for non-mathematicians. The formal statement of axiom 1 gives only a slight indication of the demands made on the reader in this respect.

However, if one can meet this challenge, the result is most rewarding, for Luce shows that his axioms imply a ratio scale for judgments (choices) which can be employed in diverse ways. The fact that choices have to be made in nearly all fields of human behaviour gives such a scale a large number of applications, some of which are explored in the book. Thus, a psychological judgment can be construed as a choice between a pair of stimuli (is A louder than B?), a gamble involves a choice amongst alternatives, as do learned responses to sets of stimuli. The basic theory can be interpreted in different ways, usually with additional restraints appropriate to each field of application, and these applications lead in

most cases to predictions about choice behaviour, some of which are not predictions which could be made (for instance on a "commonsense" basis) outside the theory. Hence, the theory has empirical content and is testable, although to test most of the predictions made would be a laborious process; several experimental results are quoted from the literature which are consistent with the theory, and one or two examples are given where results are contrary to expectation (in these cases possible reasons are put forward for the discrepancies). The main beauty of this work, however—as was indicated above—is its powerful demonstration of the wide number of possible applications of a theory of choice based on a set of simple axioms. In this respect Luce has produced a genuine theory, that is, it is formal, being specified independently of any interpretation, and the predictions are arrived at deductively. This is a great advance over many so-called theories which simply re-describe observables in a novel—and often misleading—terminology. Luce's treatment of stochastic models for learning is illuminating in this respect.

It would be difficult to assess the probable impact of this book on theoretical psychology; its austere style and the liberal use of mathematical notation will no doubt restrict its public, but one hopes that these factors will not entirely obscure its very great merits. As E. L. Walker wrote in a recent *Annual Review*, "Psychologists, even old ones, had better accelerate their study of mathematics." *Individual Choice Behavior* is a good illustration of the pertinence of this prescription.

P. C. Dodwell

Queen's University

The Clinical Application of Projective Drawings. By Emanuel F. Hammer with the collaboration of 11 authorities in the field. Springfield, Ill.: Charles C. Thomas, 1958. Pp. xxviii, 663. $14.75.

This is the first general textbook on the clinical application of all the projective drawing techniques currently in use. Buck's "House-Tree-Person" test, Machover's "Draw-a-person" test, and many others are discussed, including somewhat esoteric procedures such as the "Draw-a-Person-in-the-rain" technique, and the Hammer "Draw-a-member-of-a-minority-group" test.

This volume contains chapters by eleven other contributors, including Lauretta Bender, Florence Halpern, Molly Harrower, Paul Schilder, Karen Machover, and Edwin S. Schneiderman. By and large, the contributions appear to have been carefully co-ordinated, and the repetitions which commonly occur in books of this sort are minimized. Nevertheless, as a drawing test manual, it is unbalanced. Too little space is taken up

with the practical problems of the method of administering the tests discussed, and the details of their interpretation. The beginner would need to do considerable supplementary reading were he desirous of acquiring these techniques. On the other hand, a great deal of space is taken up with a few individual case studies, and an analysis of the drawings obtained from each.

Several of the contributors discuss briefly whether clinical psychology is an art or a science. It is fair to say that the majority believe it to be largely an art. Their discussion of drawing techniques thus centres around their personal opinions, and their "clinical experience." Generally speaking, no objective validation data are offered in support of their beliefs.

There is, however, one section by Everett Heidgerd devoted to a general discussion of the validity of drawing tests, with a short review of the literature. Heidgerd, like the other contributors, regards these techniques as "personality" tests. He does not discuss what he means by the term "personality." It is usually taken to mean the sum of individual differences, so that the term "psychological tests" would be equally appropriate. It is clear that the tests are conceived of as being somehow relevant to a wide range of behaviour, as the "validation" studies quoted are heterogeneous in the extreme. For example, studies which demonstrate that the age and sex of the subjects significantly influence the drawings made, and studies which show that fat people draw fatter people than thin people, are all held to be relevant to the "validity" of the tests. In fairness to Heidgerd, he concludes that these tests have not yet been shown to be valid. Nevertheless, he completely begs the question of what the tests are supposed to be valid *for*. If their purpose is left so vague, validation becomes impossible to discuss.

Most of the contributors emphasize the practical value of these procedures. Many are willing to accept that they have not been validated in any scientific sense. However, they feel this does not detract from their usefulness. Yet many readers will ask, "useful for what?" Are these tests intended to be used to predict how a specific psychiatrist might diagnose a patient (thus predicting the behaviour of the psychiatrist rather than the patient)? Are they to tell us what unconscious conflicts motivate patients? Are they to be used prognostically? Can they be used to determine what type of treatment will cure certain mental disorders in certain patients? Most of the contributors remain vague on these more practical issues, and when they are discussed, no evidence is presented which supports the claims made.

R. W. PAYNE

Queen's University

"*LE CAT.*" By PHILIPPE MÜLLER. Berne and Stuttgart: Hans Huber Publishers [Distributed in Canada by Intercontinental Medical Book Corporation]. 1958. Pp. 146. $3.70.

ONCE MORE, a Swiss psychologist has managed the difficult reconciliation of "European" biogenetic theory and "American" operationalism in the study of the child. Sensitive to the clinical resourcefulness of Bellak's Children's Apperception Test ("CAT") Philippe Müller introduces this instrument to his French speaking readers with an admirable attitude of rigorous scientific caution.

Even from the first page, the reader realizes that Müller's enthusiastic acceptance of the "CAT" is certainly not founded on a credulous ingestion of the test's advertising nor even of the watered-down psychoanalytic rationales that have been suggested for it in the U.S.A. Müller questions the direct applicability to Swiss children of both norms and purported meanings of the individual cards. Results of a pilot study carried through with the assistance of the National Fund for Scientific Research, the state of Neuchâtel, and the regional Neuchâtel Normal School are carefully compared to those of similar projects in and out of Switzerland to demonstrate the necessity of establishing lists of "popular" responses, typical of local groups. It is this normative material which in turn allows Müller to cast doubt on many of the assumed dynamic under-pinnings of the situations depicted in the cards.

In the first part of his report, Müller states and defends his position on a number of questions implied in the use of projective techniques for the study of personality. For example, the author destroys with bold rationality the myth of pure empiricism to suggest that all observation is selective and that any "suppositive evidence," for a theory, is in fact, a function of the observer's biased manipulation of the experimental situation. True to his premise, Müller then chooses among the many contemporary approaches to the study of child development that of Piaget as his guiding light and offers his work as a first step in establishing the objectivity of techniques which could be used to rediscover in individuals Piaget's conception of mental development. In another instance, Müller wrestles with the apparent opposition between the deterministic concept of personality suggested by the "CAT's" interpretation and the traditional concept of freedom of choice.

The second part is a critically revised manual of administration of Aron's modification of the test. The complexity of such an undertaking is well taken care of by the variety of typographical layout used for scoring symbols, illustrations, recommendations, or comments.

In the third part the reader is given a detailed description of a pilot

study conducted in the Neuchâtel schools. Müller is rightly cautious in presenting his breakdown of results in view of the small number (40) of subjects available and the changes that obtained in the testing situations when retesting was done. Details of administration, scoring technique and measures of reliability are conscientiously reported. Quantitative results are reported only for the "*variables principales*" because of the low frequencies of occurrence of "secondary," "hero descriptive" and "outcome" scoring categories.

Extensive comparisons are then made between the data for the total group of eight-year-old boys and girls and overt behavioural manifestations of the motivations usually ascribed to this age level. Differences between the sexes, and between the eight- and nine-year-olds are then explored. The high degree of agreement between the inferences drawn from these statistical tabulations (given *in extenso* in the appendices) and behaviour can then be interpreted in two ways: that the technique is a valid mirror of development, and that traditional ideas about these groups are once more confirmed. It might be noted that Müller is quite aware that many of the observed differences in score frequencies for individual scoring items may not be statistically significant, but, they do seem to reflect distinct trait patterns when taken as clusters. Some hypotheses are simply left for validation with larger *N*s.

In his conclusions Müller stresses the need for replication and cross-validation of his findings and further refinement.

Philippe Müller's "LE CAT" is in many ways a model of good and interesting research reporting. The author's elegant style and precise terminology serve him well to report clearly and interpret meaning fully.

GILLES J. CHAGNON

University of Ottawa

CLINICAL PSYCHOLOGIST

required for

Mental Health Services

VANCOUVER–B.C. Civil Service

Salary $605–$695 per month. Duties include providing consultant service to senior officials in the Mental Health Services; analysing and evaluating training and research programmes; maintaining contacts with universities, other agencies and community mental health organizations, and related work. Applicants must be Canadian citizens or British subjects with a Ph.D. in Clinical Psychology; extensive related experience; administrative experience. For application forms apply IMMEDIATELY to The Personnel Officer, B.C. Civil Service Commission, ESSONDALE, B.C.

COMPETITION NO. 61: 200.